PANEL IX
INTELLIGENCE AND INFORMATION

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Lasswell: Project 9:2, Content Analysis, is formulated as a research planning area. Content analysis is a relatively simple procedure in basic conception. The complexities arise when you are wrestling with the problem of making it a description of a variety of detailed modes of communication -- e.g., comparing the content of newspapers with printed magazines, or relating the description of printed material to the description of the flow of verbal material over broadcasts, etc. What is valid about this project appears when we ask, "What can you learn by the use of content analysis that is of any importance for an appraisal of the enemy?"

Kaplan: I'd like to suggest at least two directions in which problems lie that might conceivably be dealt with by content analysis. One is the characteristics of the Russian mass, particularly with reference to the kinds of appeals that can most effectively be made, by their own elite or by ours or by others. It might be possible through content analysis to arrive at some clearer understanding of this sort of problem in terms of the kinds of communications which reach large numbers of people through the various channels. One would have some conception of the sort of line which our own propaganda might most effectively take, and of the sort of line which would probably be taken by the Russian elite in dealing with its own masses under particular conditions. The second problem area for which this tool might be relevant is that of arriving at some understanding of elite intentions. One could conceivably infer from the content of the mass media in Russia what the expectations and demands are, and are likely to be, of the Russian elite. In both of these respects we have at present an inadequate basis for judgment. Those are sufficiently important questions to make valuable inquiry that holds forth any promise of contributing to their answers. What is your supposition as to the likelihood of arriving at any useful information about elite intentions from an investigation of the content of the mass media? Can we get much light on elite intentions from analyzing what they say to the non-elite?

Lasswell: Some light may be thrown on this by investigating the history of the current war in some detail. Many efforts were made by the investigation of these mass media in many countries to make certain forecasts. It is at this time possible to obtain the somewhat scattered, but none the less fairly adequate, records of how the Germans analyzed the out-put of the Russians. It is also possible to obtain the analyses made by the French of the Russian broadcasts and materials and inferences which they drew from it -- and so on around the list of countries. It's thinkable that one of the research lines that would yield some material pertient to an evaluation of these potentials is a critical examination of inferences based on material obtained

Lasswell: from mass media, and a systematic examination of the inferences, the exploration of failures and inadequacies of forecasting procedures.

Lorimer: You are referring now, I take it, to analyses made during the war of the communications of the enemy elite to its members. The concern was with factors bearing on the strategic strength and plans of the enemy. Our interest at the present time in the analysis through mass communication of the minds of the elite with which we are particularly concerned, Russia, is directed to two problems which are rather different in character. One is the question of any possible preparation for war. The second possibility might be to find a basis for the planning of effective propaganda, by revealing areas of tension or danger in the internal situation. It might be that one could get in these long-term operations, during a long period of peace, considerably more insight into what are the motives and operations of the elite in their manipulations of political, economic and social matters. Then one would gain in trying to understand their strategic operations in a period of war. This analysis of the lines to which the Russian masses are susceptible or with which they are familiar would be very basic in the planning of propaganda. We have to work with reference to the purpose for which we are seeking the information.

Ridenour: The value of the analysis obviously increases greatly with its age. The more experience there is, the more those using the information which comes from the analysis will know of its possibilities, limitations and holes. In order to make such an evaluation of what can and cannot be done in this fashion, it will certainly be important to arrange access for the people performing the content analysis to all sources of collective information about performance -- because it will be very difficult at first to make any evaluations at all, simply because of ignorance of what has happened as compared with the estimate made and what was intended.

Henderson: It's important to recognize that it is a long way from collecting the information from content analysis to making effective use of it. There has been some talk of making use of this material for propaganda purposes; it seems to me this is just one way in which it can be used. I agree that the further back you go and the more careful analysis you can make of a long succession of events, the more likely your conclusions are to be useful. The point Lorimer made about analyzing the behavior of the elite in peacetime is very important.

Helmer: There might be three purposes served by peacetime content analysis of Russian internal propaganda: a contribution to the problem of enhancing our ability to determine when a major war is imminent: a contribution to the understanding of the critical tensions within the U.S.S.R., as indicating critical points at which propaganda might be directed during peace time; some insight into how the gyrations of the party line are motivated and determined, to serve as a basis for more effective content analysis in case of war.

Berelson: We had experience in this sort of activity during the war in the Foreign Broadcast Intelligence Service, which had the function (among other things) of making content analyses of enemy broadcasts beamed usually to this hemisphere. I would welcome in many ways seeing some evaluation done of the use of content analysis in that work as suggested by Project 9:2a (Intelligence Value of Content Analysis). If one understands the term content analysis here in the sense of the quantitative analysis of the flow of communication by a set of systematic categories, I think it would be

Berelson: a reasonably fair thing to say that the utility of that sort of analysis was relatively low. This work began with very rigorous methods of that sort, but the agencies to whom reports were sent found for one reason or another that they couldn't make much use of the kind of materials which had been supplied. Little by little the reports changed into abstracting with interpretation -- reporting rather than systematic analysis. For some of the purposes listed in 9:2a, probably content analysis could still be quite useful. There have been some efforts to predict enemy intentions, even military intentions, with content analysis. I, personally, don't feel that such efforts by and large have been very successful. Usually content analysis was used to supplement other sources of information. The limited experience I have had with it is that, if it confirms other information it is useful, and if it doesn't then the other information takes precedence. Its independent contribution is relatively less. It is again my personal judgment that the methods of inference from such quantitative analysis have not been sufficiently thoroughly worked out. I have a feeling that our own analyses then did not use sound or even consistent methods of making inferences from changes in frequency of occurrence of these categories.

Lasswell: The essential point is the one which Ridenour emphasized, that the problem is to get experience and constantly evaluate its relevance to the problems that you have in mind. It seems to me to be very clear that procedures of this kind can supply intelligence as a basis for several kinds of decisions. The intelligence function can be described in terms of what comes to the focus of attention of the policy-maker on the basis of which he makes up his mind. Our problem is how to organize his focus of attention in such a way as to increase the likelihood that he will arrive at rational choices. The usefulness of content analysis can be described in terms of each of the major kind of choices that need to be made. For military purposes, certain of these choices have already been indicated. You have constantly the problem of evaluating the intentions of your antagonists, the potentiality of your allies, what you can expect from your own people. Your decisions are affected to a very large extent by those considerations. A flow of information of the sort which we are describing can frequently serve as a basis for a variety of inferences of this sort. The war experience has, in many countries, provided an extremely interesting lot of material. For instance, Mr. Churchill received during the first months of the war a summary of the broadcasts addressed by the German Government to its own public, to the neutral public, and to the enemy public. This material was summarized for a few months, and Mr. Churchill, it appears, read this with some attention. In the case of other Governments material of this kind did not get to the focus of attention of people at that level of decision making. Apparently from country to country and from time to time during the war, there was an extraordinary contrast in the sophistication of the techniques employed in the routing of the material to the notice of the decision maker. While the evaluations of past experiences would have a certain relevance, I think that here is one of those cases where a certain amount of imagination is useful in thinking out the problem of what there is about the events accessible to you which can be described in terms of content analysis. What is there in terms of continuing trend records that would be of some significance to you in evaluating the alternatives open to you throughout this entire range? What are the processes by which you can get these procedures smoothly articulated in the decision making process? You've got to train people to ask good questions. You've got to train people to evaluate quickly what is said to them by their advisors, interpreters of this flow of intelligence. All of those are problems which are relevant to us, and which could be spelled out with some degree of detail.

Ridenour: If content analysis were to be engaged in during peacetime by intelligence agencies of this Government, it would be a fairly extensive and expensive undertaking. What perhaps would be most useful is an analysis of what has been done during the last war when it was intensively done by many people. Is this a technique which it is worth representing as desirable to an intelligence agency operating in the United States in peacetime on a budget which the Republicans wish to cut?

Lorimer: I would again emphasize the point that this might have terrifically tremendous value during the so-called cold war.

Ridenour: Yes, and it might not, as we have just found out.

Lorimer: It might have had very little or no value during the shooting war. When we were primarily concerned with discovering attack plans, resources, etc., it might give a negative result. But if we are concerned with the problem of preventing war or preparing for war, it might have far greater relevancy.

Ridenour: In the study to be conducted by Monday morning quarterbacks, attention should not be paid so much to inferences which were in fact drawn from content analysis as to what information, in the light of subsequent events, we could have drawn from what we had available if we had used proper techniques. The difference between war and peace is far less important than the availability of an immense amount of data gathered in war time, as against the three or four years that would be required to gather such an amount now, and the somewhat smaller chance now, perhaps, of checking inferences against facts.

Lasswell: The sort of work already going forward through the regularly established agencies of Government consists, I am afraid, of the routine reading of a great deal of printed material, carried on and coordinated through Central Intelligence. There is a certain amount of monitoring and description of broadcast material. The procedures made use of vary all the way from a quick impressionistic remark to reports describing trends in the presentation of various party leaders outside the country, which give our diplomacy some clues as to the diplomatic moves which can be anticipated, etc. The probability is that our procedures are far shabbier than necessary. Persons with an adequate understanding of the possibilities of this sort of thing, can, I think, point out in many instances how the procedures can be more effectively utilized. One of the possibilities here which we might consider is the evaluation of some of the procedures which are in current practice. But I want to emphasize again the point which Ridenour just made. Suppose you were confronted with the problem of making such an evaluation. It is true that if you are bright, imaginative and pertinent, you will no doubt be able to make a number of ingenious and relevant suggestions, a substantial number of which may turn out to be quite well founded. The problem would be to spell out the opportunities that were missed, with a view to specifying how the process could be made more effective. But at the present time some funds are used by the intelligence services for the survey and description of material.

Wallis: Isn't it sometimes just as important to look into the technique used? That is, don't say we missed this factor which was important and concentrated on that one that wasn't, but ask, what was it about our technique that made us concentrate on the unimportant one?

Lorimer: I think there might be very great value in this suggestion of the analyses of the war time procedures. But I wish to register a warning that the finding of relatively low value in the war time procedures directed toward strategic war time problems would not necessarily indicate a low value of somewhat similar procedures applied to diplomatic and propaganda problems.

Lasswell: An evaluation of what went on during the war in the United States and in other countries to which we have access would provide a great deal of material that is pertinent to a state of cold war or crisis. We did have the problem of our relationship with non-belligerent countries, and we had also plenty of examples of the use of material for the making of diplomatic decisions. We had also many instances of these for material addressed to a large number of people in our own country, neutral countries, our allies, and the enemy. And the choice of material to be sent out was influenced to some extent by intelligence about what was current in their channels of communications. So an investigation of war situations would provide a very wide range of situations on the basis of which we could discuss the types of problems which are current in a crisis and semi-crisis situation, where one always has to cope with the problems of censorship and deliberate control of monopolized flows of communication. I don't want to minimize, however, the point which you are making, Lorimer, because I agree that the relevance of content analysis in crisis periods is no less high, and for some purposes even higher, than it is when the situation is completely tight. But my guess is that it is applicable all the time and that the extent to which it is applicable, and by which procedures, can be clarified by the kind of inquiry which we have been talking about.

Wallis: If you were running one of these content analysis surveys in peace time, you might well analyze the data in exactly the way that a manufacturing engineer controls the quality of the product. He sets up some kind of a control chart and allows for limits of variation that he can expect in his quality if the process stays the same. From past experience he learns what to expect, and uses his data to spot a point that's "out of control". If he doesn't have this control chart he thinks maybe it is a random variation, maybe it is something significant. He sets up these limits to tell him when a process is supposedly "out of control"; he says in those cases that there must have been a change in the basic system that is producing the results. One of the things that impresses me about this is the possibility of getting some idea of what we can expect of any particular country in the way of variations, knowing when to look for changes, and having something more than an impression. Presumably the whole point of content analysis is that instead of listening and then giving an impression of what was emphasized, you actually classify and count and are able to make it objective.

Lasswell: Had content analysis procedures been in operation in 1938 and 1939, the attention of people observing Russia would have been called to the fact that there was a gradual dropping off in the number of references of a neutral or favorable character to Britain and Germany. Everybody had his mind so focused on the reiteration of the negative references to Nazi Germany, that little attention was paid to the drop off in the curve of references to Britain -- which would have served as a warning question mark at a fairly early point. I'm calling attention only to the fact that some descriptions of some contents of communication have indicated the probability that there were certain curves, which, if they had been kept in proper trend fashion and analyzed in various ways, would have signalized something worth looking at and

Lasswell: trying to interpret with a great deal of vigor. There was no such systematic procedure in operation at that time. This is the sort of thing which would have the minimum warning light effect.

Kaplan: One of the directions in which it might be very fruitful to turn content analysis is with reference to the notorious gyrations of the Communist party lines. Whether it is possible to do so by consideration of the mass media, I don't know; it might very well require media of much more limited circulation among the elite or sub-elite circles. It is perfectly reasonable, I take it, to assume that those gyrations are not random but the expression of a response, given certain predispositions, to the stimulus as the Russian elite sees it. It would be enormously useful, and just as possible in this instance as in some of the other that we have considered, to be able to anticipate, if not the direction of a forthcoming gyration, at any rate its occurrence. For example, consider the situation of the United States a short time ago when the leadership of the American Communist party was completely transformed. I am curious as to whether it might not have been possible, by a close scrutiny of the various documents and papers and bulletins which had been issued, to predict that some change in leadership, or at any rate, some determined effort to impose such a change, might not have been forthcoming. It might be useful, therefore, in the survey of the possibilities of applications, to pay attention not only to relations between elite and mass or among elites of different countries, but also to see what could be done with regard to internal elite changes.

Henderson: I should like to emphasize what has just been said by Berelson and Wallis, because I think the two things tie together. You can have an organization collecting and making elementary analysis of certain information. Then the question of what happens to it comes up. You need perhaps a different type of personnel to continue the analysis after you have noted some of the warning lights about a major change. This brings up the whole question of how you handle the material, how it is made available to the decision makers. This is also very important with respect to this abstracting proposal, which, I take it, would be most important in the case of scientific and technical publications. Here there is more chance, perhaps, of getting reliable factual information which would reflect the policy of the elite along certain technological lines, to give you a fair indication of actual planning for peace or war.

Lorimer: Assuming we have a period of quite a few years to operate, would it be worth while to maintain for a time as an experiment some teams following different methods, operating in isolation, in order to compare the results obtained by entirely different techniques of analysis, to see whether there was a convergence in their results? Another suggestion is that one must constantly bear in mind the terrific segregation of knowledge within Russia. I assume that most Russian Government officials, even on very high levels, do not know the intentions of the inner-control group. Perhaps specific propaganda agencies might come nearer to being tipped off to what directions to take. But there is the problem that most Russians don't know what the Russian line is.

Kaplan: As to the first of Lorimer's suggestions, checking on the reliability of content analysis procedures by carrying them on concurrently with other methods, I know of one anthropological investigation which is now going forward, although on a very small scale, quite along the lines Lorimer was suggesting; it strikes me as an

Kaplan: extremely useful one. Some questions would certainly arise as to how validly one may extend the results from that kind of an investigation to the sort in which we may be interested. The problem worked on there is a study primarily of attitudes of certain Indian groups in this country. The anthropologists are applying concurrently but independently the standard types of anthropological participant observation, a set of conventional projective techniques, thematic apperception tests, etc., and then, in addition, a content analysis (possibly not strictly in the sense we have been using the term today but one not far removed from it) of folklore and other materials in that culture. It would presumably be very difficult from a practical standpoint to obtain any such direct checks of the reliability of content analyses with regard to inferences about attitudes of the Russian people. But it may be possible by other such investigations to establish a body of material in terms of which one could say that what you obtain by an analysis by the flow of communications yields within such and such limits of error the same results as would be obtained by far more intensive procedures. This would be an enormously valuable kind of information to have.

Berelson: I would also say, with reference to Lorimer's first point, that in a sense that is what happens in a concrete situation, if we are speaking about the validity of content analysis contributions to intelligence, because it is constantly being evaluated against all sorts of other kinds of information about the same area. That's an altogether desirable thing to be done. On applications of content analysis to certain USSR problems, there has been another suggestion made: that content analysis be used in analyzing the reactions of various Soviet publicists, representatives of certain groups, and people writing for certain groups, to U.S. - USSR relations. The next time something like the Marshall Plan or the Truman Doctrine is proposed, all these Soviet publications will respond to it editorially. The suggestion has been made that rigorous analysis of the differential responses may be able to identify those parts of the elite which are more threatening to the U.S. and those which are more friendly.

Lasswell: If investigations were carried out on the use of content analysis in concrete situations, they should certainly include a study of the sort of personnel best adapted to the performance of the several functions involved in the procedure. The point which Henderson made is particularly important, since many times the implications of a given content analysis procedure have not been fully realized, because the material has not been presented to people who had the predispositions that would make it possible for them to see its significance for the larger context. Sometimes even an item like a death notice may be important. If in certain areas the reported deaths have suddenly dropped near zero, the implication may be that the deaths in that area have increased to such a point that precautions are being taken to censor or suppress this type of material. The interpretation of whatever is spread along the line at any given point calls for a systematic exposure of details to those aware of the different frames of reference which are pertinent to its total evaluation in the context. Those are the kinds of refined problems which arise when you get serious about intelligence operations and begin to think of each step in the game and the way in which the appropriate personnel can be correctly articulated at each step to realize its full potential.

Wallis: A crucial person on the team is a statistician. You are going to get into rather complicated statistical problems, those of multivariant analysis.

Lasswell: It's within our scope to consider counter-intelligence operations, in the sense of providing the decision makers elsewhere with information which will increase the probability that they will make decisions favorable to the continuance of peace. The problem is how can we use certain kinds of channels for the purpose of getting certain effects. There arises the question of the use of various means for the deception of enemy leaders, for instance, the question of the circumstances in which deception could be most adequately carried out, etc.

Berelson: Projects 9:2b and 2c, on intelligence from indirect or remote correlations, demand someone with the qualities of Superman. What is described here is scarcely the kind of thing that can be generated in the first instance by means of a formal project, although the utility of suggestions can be checked on a formal basis. What is required is simply that a sufficient number of extremely intelligent people think hard about this for quite a while.

Wallis: There is a wonderful and very simple case of the value of indirect sources of intelligence. On many crucial military items we knew before the Germans did what their rate of production was. They waited until they got all the figures sent in to the central office and then tabulated them, whereas we took samples by capturing them on the front and made a sample analysis. In some cases they never did find out exactly, as we did, what their total production had been. This is notably true on the V-2. Our figures hadn't any errors at all from month to month. The only uncertainty was how many they produced before they fired the first one.

Berelson: Our attitude is a declaration of interest in so far as the generating of suggestions is concerned. Given some suggestions, it is a project matter to determine their worth. The real point of the project is to give some advice to, say, the central intelligence group. Espionage isn't effectively carried on by spies any more.

Lasswell: Could we ask ourselves what we know about the effective organization of intelligence processes in different situations? Have we brought to bear our best psychological and social science techniques on the problem of evaluating successes and failures of intelligence set-ups under different circumstances? My own guess, based on examination of some of the appraisals of intelligence operations which have been made available to some of our Armed Forces, is that those appraisals are on an extremely unsatisfactory level, and that it would be possible by the use of appropriately qualified personnel to carry through an examination of the various factors which increase the probability that the intelligence process will function efficiently. The fundamental question is how you organize an intelligence process in a given power set-up to increase the likelihood that you get the kinds of people at certain critical places that will give a realistic description of trends and potentialities, assist in the clarification of objectives, and in choosing the relevant one. The answer sometimes given to this question is that you hire a smart man to run it and leave him alone, which is not the arrangement we have in this country. However, the specification of a smart man leaves him the problem, among others, of deciding in the light of anybody's past experience what kinds of choices are open to him in solving his organizational problems. One of the questions here is whether or not we need an analysis of past experience which would make concisely available to him the successful solutions of that type of problem.

Wallis: The project could be broadened to cover not only this point you make of how

Wallis: it will be set up but what kinds of information an intelligence group should aim to get. There probably is, to some extent at least, a traditional attitude in intelligence that they collect military information or they spy on diplomats. We might go into both the question of organization and the question of what constitutes intelligence relative to modern warfare.

Berelson: As a point under what Wallis just said, what is needed is some comment on what kind of information is desirable. Then, perhaps, some remote correlation could be figured out in order to get it. Setting up such a project without a problem base, or a content base, would be relatively less effective.

Ridenour: The point made by Wallis is extremely important, and one that is often lost sight of in traditional intelligence work. Possibly the best way of all of securing intelligence is to have our representatives in the United Nations work very hard for international agencies which will tabulate information of all sorts regarding the various countries of the world. I am convinced that this is probably more effective, less expensive, and generally more reliable and satisfactory than any sort of intelligence work carried on in the old framework of trying to find out something which the other chap is trying to conceal. There are other ways of making available and obtaining routine information on all sorts of domestic matters which are of importance to the intelligent people, whose job -- if exchange of information were extended as far as possible -- would become one of evaluation. Project 9:3, "Social Science Aspects of an Accurate Map," is just one tiny facet of this jewel that Wallis has laid before us. The previous one, 9:2d, "Interviewing as a Source of Information on Russia," does not constitute a project by itself, unless the project is to evaluate the usefulness of what has been done for five decades by the British and the Germans and ought to be done by us. All we need to do about this is present to the right person, under the best possible auspices, a suggestion that this be done.

Lasswell: It's thinkable that something relevant along this line could emerge. What I'm thinking about would represent a contribution to the general theory of information getting. The problem is identifying which interview subject, handled in a certain way, will disgorge the most pertinent information. At the present time that usually eventuates in a series of specific instructions. The question arises whether we want to suggest a rigorous evaluation by appropriately equipped teams of psychologists, etc. The technique of interviewing would, of course, involve the problem of establishing a relationship with qualified experts to specify what kind of information is wanted and to evaluate what comes out of it.

Helmer: This would be very important. Presumably the techniques to be developed would be very different depending on whether it is a question of conducting mass interviews, where presumably the methods of opinion study would be used, or whether it is a question of individual interviews. Of these there are two kinds: It might be the sort of interview that, for example, Stassen might have conducted on his tour through Russia. (In fact he has done something of the kind, but presumably he missed many opportunities which he might have had if he had been properly instructed.) Or, it might be the kind of interview we could conduct with people returning from Russia.

Lasswell: Among the very many interesting questions is what you can get by conducting interviews under various conditions of hypnosis which may or may not be drug induced. One of the problems is the organization of our present knowledge about the relative usefulness of interviewing under those conditions for information purposes. For

Lasswell: therapeutic purposes it's now very common to conduct interviews of this kind, and when you are trying to decide what kinds of attitudes people have toward the authoritative groups in their community, what their hates and loves and disappointments and resentments are with respect to the various parts of the social structure in which they operate. One of the ways to find that out is not only by prolonged free association techniques, but by interviewing under these conditions. So far as I'm aware this type of procedure has not been systematically evaluated. Here is a place where one would need to have a team composed of appropriate physiologists, physicians, psychologists, and information specialists. I am speaking both of interviews with subjects who do not wish to disclose everything they know, and of cooperative subjects. There are at the present time some subjects who are willing to submit to interviewing under these special conditions, with the idea, as a matter of fact, that they can disclose some of the deeper characteristics of their own personality organization; there's no question of coercion involved. There is also the problem of what you do with people when, in effect, you use this as just a device of stunning them and obtaining information which they may not want to divulge. If you want to allow the Buck Rogers imagination to hold sway, for a moment, you might imagine that one of the ways to influence the responses of elite groups in any other country would be to subject their agents abroad to this type of treatment and then dismiss them with amnesia for the treatment, so that the information and attitudes with which you have indoctrinated them during this method of treatment will be one which they operate in the enemy country. Here is a possibility for the correlation of physiological and psychological knowledge on using the Russian representatives abroad to supply them with certain basic education consistent without aims.

Ridenour: This project contains at least two different things. One is merely a suggestion that the intelligence of our Government would be helped by a systematic program of interviewing people who have been in foreign countries. This is readily set up, does not cost much, and probably will have heavy yield. The second is the suggestion you just made in detail, which seems to me to be an excellent project for a qualified team to carry out.

Wallis: The project is useful in time of war with captured prisoners as well as in a pre-war period.

Lorimer: It might be worth investigating the effectiveness of the methods of interviewing as carried out in other countries during recent years. This might include the whole range of questions about how to obtain information from individuals, directly or indirectly. It might include also some simple operational analysis, tabulating simple information about individuals that could be asked on routine forms without creating any suspicion, e.g., finding out their age and technical training; if they're Russians, keeping track of where they go in this country; and if they're Americans sent for by the Russians or otherwise, or others on their own business finding out how they happened to send for those persons and what they do with them over there. I wonder if there is any single agency in the country that has now all of that information. Is there any one now which would have a list of all travelers to and from Russia in the United States, together with some information about their occupation and length of their stay?

Lasswell: It's unlikely that the time details would be accurately recorded, but I think that there are lists that are approximately complete.

Lorimer: Is there any information on how much money they bring in with them, how much they take out, funds at their disposal, etc.? There are so many restrictions on international travel now that such data could all be easily obtained.

Lasswell: Yes.

Lorimer: The group of German and possibly Japanese prisoners being returned from Russia is perhaps one with which systematic interviews would be worth while.

Berelson: On the problem of how best to train people to secure information within other countries from whatever sources, I have been told (it's a little hard to believe but perhaps it's true) that some people who are trained for this purpose were able to do an amateurish public opinion poll in Russia by interviewing people in the different parts of a town according to some sort of sampling procedure.

Lasswell: If we decide to investigate the organization of an intelligence function it would certainly include the problem of selection and training of personnel appropriate to different tasks; Berelson's suggestion would be included in such an investigation. There's another line of closely related possibilities. One of the major possibilities which RAND may want to take into consideration is this: We need to think in terms of a continuing set of institutions which provide us with the knowledge judged to be the most useful for policy. It's entirely thinkable that this could be stimulated by the process of assigning integrators to different regions of the world. The function of the integrator would be to maintain a continuing survey of the adequacy of our intelligence with respect to different areas. His job would be, at the same time, to come up with ideas about more effective ways to make use of scientific and other personnel for these purposes. It's plain that we can have sample community studies made throughout the accessible world, not only by those officially designated, but by private scholars who, in the conduct of their regular scientific work, are interested in carrying on these community studies. It's entirely possible that a research integrator can work out the ways to find the people who could most expeditiously be used to carry on those investigations.

Wallis: This is the sort of thing that Congress would have a right to know about but which, if publicized, would be defeated. We might investigate how the British Secret Service has operated with respect to Parliament in the past, although they haven't tried this kind of thing.

Lasswell: What I had in mind is something which could be set up very harmlessly in the existing framework. It's not necessary for anybody to be called a research integrator or coordinator; he can be a staff member whose job is to find out who is appropriate for different enterprises and to exercise his good offices to open up facilities. It should be presented in such a way that Congress would feel that RAND, for example, would be derelict in its duty if it failed to keep itself constantly informed of those scientists in the nation at large who had knowledge pertinent to their problems.

Wallis: I wasn't worried of Congress disapproving of it, but rather of their publishing it in the transactions and it then being read in Russia.

Lasswell: The Russians are so systematically suspicious that even should there be

Lasswell: certain publication, it would scarcely add to their surprise. But I should imagine that it would be possible for an enterprise of this kind to be conducted in a thoroughly clear and aboveboard fashion. There could be news leaks of a certain type about this undertaking without doing damage. I wasn't thinking of anything insidious, but of something genuinely integrative of our resources.

Lorimer: It's important to emphasize once more that it is only the last evaluation stage of intelligence procedure which represents something that cannot usefully be printed, circulated very widely. All we know about Russian uranium now is in some geological year book of 1943. Despite very vigorous efforts of all our snoopers, the dope is there in the book, and that's all there is. I think this is typical of the approach to intelligence.

Henderson: I wonder whether it wouldn't be surpressed by foreign countries if they knew its object. Of course, we can do that sort of thing now in, say, the British Empire. We have done so on commercial enterprises. Commercial attaches are there for a similar purpose. In countries of that sort they are welcome, and they keep their people here. But I wonder whether it wouldn't pretty much hamstring the whole affair if the Russians knew its objective. They already suspect our commercial attaches of doing things like that.

Lorimer: The reason it will have no effect on Russia in this matter is that any person giving any economic or scientific information outside of Russia is liable to punishment. The only difficulty with the published information (and I thoroughly share the sense of its frequently greater value than snoop stuff) is that the statistical material -- birth rates, death rates, production indexes, etc. -- is not available. At the present time the Russian Government is leaving the lines blank in all the tables of the statistical bulletin of the United Nations. The Russian Government is proceeding on the assumption that we are utilizing every bit of information. I think there is no problem, as far as Russia is concerned, of creating a suspicion; we couldn't possibly deepen the suspicion they now have. To deceive the enemy is critical in war time. Our problem at the present time is to develop a technique by which Russia could keep from being deceived about the intentions of our leaders. The center of the atomic control problem may be that American intentions are mistakenly interpreted.

Lasswell: I would like to register the hope that it would be possible for our physical science specialists to come up with means of communication that would make it possible for us to reach large numbers of people in the Russian audience by methods which could not be stopped by other ruling groups -- in short, something like non-stoppable radio. I'm not going to try to indicate whether there's the faintest possibility that something of this kind could be a reality; what I'm talking about here is what one would like to have. Suppose we do want to reach large groups of Russians, for example, in order to clarify what we conceive to be the fact that we are not poised to attack Russia. Is it possible to develop instrumentalities of a technical sort by which we can maintain a daily and incessant stream of contact with Russia?

Lorimer: There is a possibility of propaganda to strengthen democratic forces, democratic tensions, within the U.S.S.R. The possibility of strengthening the suppressed potential democratic forces is a very interesting project, and it turns a good deal on the possibility of any way of infiltrating information. For example, if groups of Soviet citizens had been invited to attend conferences in the United States and their

Lorimer: Government has not allowed them to come -- is there any way that information could be gotten to those individuals so that they would know that they had been invited but that their Government didn't want them to receive the invitation? Similarly, is there any way in which information can be gotten to Russian masses?

Lasswell: The suggestion here under 9:1a is a beautiful example of the kind of project that points to the need for a continuing survey of certain relationships in and around the Soviet Union. It is reasonably clear that one of the ways to get information to Russia is by bringing it to the notice of Russian sympathizers in the intermediate countries, who then, in many instances, through their network of personal contacts, are in a position to relay it. However, these relationships change from time to time; as channels get blocked off it becomes less easy to communicate in this indirect fashion effectively. But there are Russian representatives, official and unofficial, and there are friends of those officials in all countries of the world. One of our ways of reaching Russia to a very considerable extent is by reaching those people outside the United States. And one of the problems as to how to do that is to maintain a continuing survey of the state of those channels open to us for doing this. Here is where the organization of the procedure that would keep our information of these channel potentials up to date seems to me to be particularly important. It would not be at all difficult in any country in the world to carry through an investigation of the channels of contact between (a) persons directly connected with the Soviet Union, (b) political satellite agents, (c) locals who are in some form of face-to-face direct contact. Analyses can be made of the opportunities for getting to the focus of attention of each of those rings of people. To some extent this involves selectively putting certain materials in the newspapers, the gossip sheets, and in the scientific society and engineering society communications in, say, Cairo, which at the present time is a particularly convenient sieve into the Soviet Union for some purposes. I would think that this would be the way that one would analyze the project. It would be the problem of examining the going network of contacts between one group of people and another. It includes all of these intermediate contacts and the ways by which what gets to the focus of attention of some of these persons can be appropriately organized in ways that we want.

Wallis: Project 6:3 on a Belligerency Index concerns essentially an intelligence operation. In Panel 6 Neumann was rather emphatically opposed to this; he said that it could be proved that such an index is impossible.

Ridenour: The discussion in Panel 6 centered largely on the objection that the preparation of such a belligerency index might be taken as a substitute for thought.

Lasswell: The problem is largely one of making sure that whatever figures you got, you presented to intelligent people in such a way that their relevancy is clear.

Wallis: The idea apparently was taken from Richardson's monograph on Generalized Foreign Politics. The feeling of those who read the monograph was that it was not promising as a method of getting an index of belligerency.

Lasswell: It is reasonably clear that as a community moves toward or away from an intensified crisis it's quite likely that some of its characteristics will change. Our problem is to find that out, I suppose, by the process of collecting those describable details and subjecting them to continuous rigorous criticism.

Wallis: The discussions in Panel 6 were largely about the possibility of getting anything that you would really call an index -- that is, a quantitative measure. That might be possible, statistically, if you had a large volume of information. But with relatively few cases of war breaking out, you certainly couldn't establish on any statistical basis the validity of such an index. But, as you say, you might by an analytical process establish at least a presumption from a mass of data.

Lasswell: There are a good many hypothesis that could be formulated whose operational indexes are likely to hold good for rather limited historical periods -- hypotheses about the way in which a society with a certain kind of social structure will behave as it moves into crisis and away from it. Among many characteristics, I suggest that there will be a change in the volume of demands which are given publicity by subordinate groups. More specifically what I mean is this. If one inspects the entire media of Russian communication, or more properly, samples it with respect to significant groupings in the Russian community, one will discover changes in the amount of publicity referring to the demands made by different kinds of occupational and other groups. As you move into a very sharp crisis situation there will be a rather rapid contraction in the number of demands made by and in the name of subordinate groups. I wouldn't want to suggest that much of value would come by developing some statistical summation and forecast, but a case-study of this cross section might enable one to arrive at some critical evaluation of what was going on.

Wallis: This coincides with our discussion of last night, I should say.

Berelson: The research proposal for a study of techniques of interviewing seems to me to be unusual in the intelligence field; it is rather clear cut and very much worth while.

Lasswell: There was favorable discussion of the possibility of evaluating the use of content analysis in the last war (Project 9:2).

Berelson: In content analysis you've got to do it on a big scale or it isn't any good. You've got to do it for a long time in order to learn how, and also in order to evaluate the results. You've got to have either foreknowledge of the future or a project which has existed for several years in order to have access to the facts that you were forecasting.

Wallis: In the first place, you could find out whether different content analyzers make anything like equivalent analyses. American sources would be good for a trial run. At any rate, you could certainly test that for whether you've got a technique at all; that, no doubt has been done in some cases. Secondly, you might study whether you get any constant pattern of variation, or whether it is so erratic that you would never be able to use it to spot a point "out of control."

Lasswell: The point that you need to have a large-scale operation is sound. But there are certain more restricted types of inquiry that are worth making at the present time. Those specific lines of inquiry would be designed, for instance, to show the interrelationship of the techniques which are available for conducting content analysis. I would like to include in that (what is especially important for RAND, I should think) the use of automatic reading devices for feeding material into a content analysis procedure. It may be that by the use of certain devices for scanning printed

Lasswell: matter of monitoring broadcasts significant symbols could be automatically recorded. My point is that in addition to comparison of procedures of varying degrees of intensiveness there is also the problem of thinking about the areas where productive technical invention might be correlated with the procedure.

Ridenour: What is objected to is making a policy recommendation that content analysis be established now and national policy based on it. But the proposals are for a series of smaller projects as a means of investigating whether there's anything in it, if so what, and how can it be used. Berelson, who has had actual experience in the field, said that the utility of content analysis is really not known. It may not have been very useful in the last war, and on the other hand it may have contained in it a great deal of usefulness which was not got out because procedures had not been properly developed. Is it not better to carry out a post mortem investigation of this body of existing data, which can be had free and at once, before embarking upon experiments in order to define the directions in which those experiments were made?

Berelson: It doesn't seem to me to be altogether a sound proposition even on a smaller scale. I agree that it would be a very useful thing to evaluate the experience which we have had to identify the area in which it is more or less useful. I would be a little sorry to see a very large scale enterprise set up now. There would be possibilities even within this area now to apply it and see what happens. For example, one could do a very simple analysis of the technical journals in physics which are coming out, and chart the currents of certain kinds of articles of which physicists could say, "If they are doing such and such work, then they have to be increasing their output of these kinds of materials." Experiment along those lines on relatively simple circumscribed projects might produce something useful.

Kaplan: Another specific illustration of what Wallis was urging are some studies carried on during the war on the reliability of some of the content analysis procedures. The results were not sufficiently well grounded for anyone to feel that no more investigations need be made in that direction. There was considerable variation in the results of the different studies, and they were all carried out on rather a small scale. It might reasonably be supposed that some other such studies of the degree to which, under specified amounts of training, different analysts would classify the content of given bodies of material in the same way would be extremely valuable. It might happen, for example, that the British procedures had a very considerable intelligence value, and we would not know whether this was due to the fact that they had some very brilliant men who were left alone, as you put it, or whether it was due to the development of a set of techniques which can be transmitted and communicated and which will reliably maintain that level of value. Pilot studies in that direction, I take it, was the sort of thing Wallis was suggesting. It could be carried out on a small scale.

Berelson: This last would certainly be all to the good. The reliability studies to which Kaplan refers were done on relatively simple sets of categories. The reliability on very simple things is quite high, but on more complex matters it's another problem.

General descriptive abstracts of a whole series of Soviet publications might be useful. As I understand it, it would be something approaching the BBC digest during the war -- which was a rather full scale job of abstracting. Primarily, I was thinking of its use for intelligence purposes; but it could also be given widespread dissemination throughout the scientific community. It may be that the purpose could be served

Berelson: by sampling procedures. There would certainly be samples appearing in the abstract from time to time of what came out of various publications. It may be that the publications themselves could finally be sampled. That was done with the BBC Digest, I have heard. They monitored and read a good deal of material, but then only a small amount of it was summarized. Suggestions along this line have been made in Washington and other places. It may be that it won't necessitate any expeditures on RAND's part, but only some firm recommendations in the right places that this be undertaken.

Lasswell: I take it we are all substantially in agreement that there should be a post mortem on the total relevancy of content analysis in the past war situation. I gather that there has been general acquiescence along that line. Are we in agreement that there's a place for technical investigations which are designed to show the reliability of the content analysis procedures themselves? The second would presumably be small-scale enterprises involving only a few thousand dollars, in contrast with the large expenditure involved in an intelligence operation adequate for national policy.

Ridenour: I'm not opposed to that; I feel only that they will probably grow usefully out of the post mortem studies. There will be points that are not clear and must be investigated.

PANEL X
MILITARY AFFAIRS

S.S. Wilks (Chairman)

A. Alchian
B. Brodie
C. Dollard
L.J. Henderson Jr.
C.J. Hitch
W.F. Ogburn
L. Ridenour
H. Speier
W. Weaver

Weaver: On 10:1a, "Military Worth," there is a considerable group in the home office staff of RAND who are concerned with attempts to see whether or not a useful concept of military worth can be constructed and given quantitative value. Everyone knows that a limited concept of military worth does exist -- you can take two computing sights and find out whether one is better than the other for a given tactical employment. The question is, to what extent can that concept be generalized, and how broadly can it be generalized? Can you ask an intermediate question which is still pretty broad, like "What should be the policy with respect to bombers; do you just keep making them bigger or don't you? What is the general policy with respect to very long range bombers and their protection?" Etc. I'm tremendously enthusiastic myself for taking this general point of view in trying to deal in a quantitative, reproducible, analytical fashion with as much of the decision making in military affairs as one can. How far it can be pushed, I don't know. I'm perfectly confident that it can be pushed much further than it has been in the past, and I think it should be under systematic study. There is a large section of RAND which is working on it. And it is, of course, being worked on very actively by a group in the Air Forces, by OEG in the Navy, etc.

Ogburn: Does this question of Military Worth concern itself with the priority of targets to be attacked? If you do think so, then one point that might be appropriate to this group would touch on the question of the defense of the civilian population. I would be interested myself in knowing what is the military worth of the lives of non-combatant civilians. Bombs, I suppose, are of two types. One is the explosive type that is likely to hit factories or objects of particularly military value, and the other type is the incendiary bomb which has for its purpose, presumably, considerable destruction of dwellings and human beings in congested areas. I have heard some say that the attacking of civilians in this manner is of considerable value; I've heard others say this is something of a rationalization to justify these attacks. Back of it all in my mind is a very wild notion that we ought to contemplate some time or other the conception of restriction of bombing, and whether it can be imposed or not. If restricted warfare and bombing is contemplated (such as declaring certain cities "open" and the like), I presume this question of military worth would come in. What it all boils down to from my approach is, "Of how much military worth is it to disorganize and destroy civilian population?"

Dollard: If in worth you also involve the question of what it costs to accomplish

Dollard: something with a certain weapon, it is worth knowing that we have suddenly discovered that in the small world we live in the victor has to put the world back together again after he destroys it. How much does it cost you to put your world back together again? That is a question that we largely ignore.

Weaver: This point raised by Dollard brings up two things. First, it brings up the next project, which is directly concerned with the characteristics of a possible system or systems which enable you to achieve your goal at minimum cost. Both the comments bring out very clearly the fact that this project is so broad as stated that we could discuss it interminably. There are various phases which can be separated out and should be studied; in particular the point Dollard raised has been under study and should be pursued further, and likewise the relation between physical destruction and other means of accomplishing your objective. Cost in the more conventional sense must be considered. If, for example, we are trying to decide whether to bomb cities or bomb factories, we are not only interested in the damage and having some definition of military worth that will enable us to compare the damage inflicted on the enemy, but we also have to take into account the cost of carrying out those two operations. I've had a little experience with this problem; I know it's tremendously complicated; it's one that ought to be pushed. In addition to the study which I spoke of in connection with target analysis, there is the obvious question of economic target analysis, which is absolutely basic in setting up target priorities. If this project is included here for us to reaffirm, at least in my case, interest and enthusiasm, it simply means do we believe in trying to carry analytical studies of warfare as far as they can be usefully carried. It would be a little difficult to take a stand against that; it would be like voting against mother-love. If it means just what do we think ought to be done about it, we are not in a position to say that. I think we would simply agree that analytical studies of the technique of warfare should be carried as far as they can, and that there should be a maximum utilization of the knowledge and techniques of the social sciences as they impinge at many points on the whole range of problems.

Speier: I might ask whether the intention of the project is to analyze the concept of military worth merely with respect to military warfare, or also with respect to, say, economic warfare or psychological warfare. Possibly in the non-military fields the social sciences can make more contributions than in the military field itself. Logically, there doesn't seem to be a reason for not assuming that one could speak of military worth with regard to certain economic war measures or psychological warfare measures, just as well as with respect to military actions.

Henderson: Project 10:1b on "Perfect Weapons" stems from some thinking and a small amount of writing which has been done by Brigadier General Maxwell of the Air Forces, who has been for some years concerned with requirements. He is quite concerned about the limitations of requirements thinking in the Air Forces, which has always been aimed at the problem of how to achieve the maximum of physical destruction. He conceived the idea that it might be worth while to set down the desirable characteristics of something the technical feasibility of which was competely unknown, and which wasn't even considered at the time of setting down the requirements. He has written a paper on this, and though I have not discussed it with the General himself, the intention, as I understand it, is to carry this further and see if it isn't possible to come out with some further refinements. It seems to me that this is pretty hard to separate from military worth.

Weaver: There are several hundred tests of military specifications that I had to read over the last six or seven years, which usually started out by saying that this device should be constructed of noncritical materials, with minimum weight, minimum volume, complete invulnerability, such that it would never go out of use through any defect of manufacture, and have perfect accuracy--period. This type of specification leaves me cold towards the concept of a perfect weapon. I think it relatively easy to write the specifications for the perfect weapon, but I don't think they are very realistic.

Henderson: I think General Maxwell feels that war has become so destructive in modern times that it certainly cannot be tolerated any longer, and that one must now examine whether destructiveness, in itself, is the desirable characteristic of a weapon or whether we had better focus our attention on weapons which enable one group to continue its diplomatic negotiations with another by means that do not destroy the economy, the population and the national wealth.

Dollard: I would be happier about both these projects if the language went somewhat as follows: to determine the factors affecting the cost, worth and efficiency of weapons in modern warfare and peacefare. Obviously that still doesn't give you an outline of research projects, but I think it states a little more clearly what you're trying to do at this stage.

Ridenour: Project 10:2 on "Peacefare Use of Military Weapons" is really a matter which conerns public relations in the large.

Wilks: What occurred to me is whether it would be possible to get reactions to our various military maneuvers, etc., not only of Soviet Russia but also of some of the Western European countries, by some kind of public opinion polling technique.

Ridenour: The suggestion here -- it might be a very useful one -- is that past performances have not always been ideal from the standpoint of public reaction here and abroad. This has very little to do with the character of weapons which this war has made, and a great deal to do with the surrounding circumstances of news release and general attitudes. So that I think this might well be worth while, though not precisely the business of this panel.

Ogburn: This is a problem in my mind. Between wars, displays of power for the purpose of obtaining objectives is a technique of diplomacy. My recollection was the Italians put on a display of air force which had the effect of frightening Britain and led to giving Italy a freer hand in North Africa than they would otherwise have had. A study of the power games that go on between wars traces back a good deal to military might. I suppose the question is "How do you display that military might?" In these days of radio and newspapers and public opinion display of military might may have some effect.

Wilks: It should be fairly easy with a lot of these polling organizations that are springing up in Europe -- Gallup affiliated organizations -- to do some polling everywhere except in Russia and the eastern countries, to find out the public sentiment of other countries. Of course, I don't know how you could ever do that for Russia and her satellites.

Ogburn: From the standpoint of history, the polling technique would not be so appli-

Ogburn: cable. Would it not be a question of the study of past crises? For instance, as I remember, before the war Germany sent a gunboat down the Mediterranean and put on a display of power there which was considerably effective.

Speier: This is a very useful project. "Public Display of Military Power in Peacetime" would be a better title. The data would include all sorts of material in addition to the results of public opinion polls of mass reactions -- for example, diplomatic protests, panic in some cases, rumors spread as a consequence, etc. I think it would be unduly restricted, even for future cases, to confine our recommendation to the employment of public opinion polls. The subject as a whole seems to me worth while exploring. As to the situations in which such display has proved effective in the past, as to the knowledge we have about the increase or decrease in national status as a consequence of such display, and as to the preference of various nations in resorting to this type of display -- I think there are a number of books that might be usefully explored.

Weaver: It would be awfully interesting to know what happens in the foreign press when we send a big carrier into the Mediterranean, when we send thirty B-29's to Germany as we did a couple of weeks ago, etc. What really happens? To what extent is it recognized? What is its impact? We obviously are acting as though that were important and usual but I don't think we know whether it is or isn't.

Dollard: What is important is the point of view not only of Russia, but of the rest of the world as well. It is probably more important to know whether that is resented by the rest of the world.

Ogburn: My guess is that it would be pretty difficult to find out. A rough index would be public opinion.

Dollard: Would it be possible to make, as Speier suggested, just a press analysis? I would have my fingers crossed as to what you would get in an attempt to do any broad press analysis. There is a survey now available of American reaction to Bikini, but that's solely American. What I had in mind was an analysis of the press of other countries. Just do it as a pilot study -- it wouldn't be terribly expensive to see what leads you'd get out of it.

Speier: In the case of totalitarian countries like Russia, a press analysis is much easier. It also foreshadows what you'd get in an opinion study -- by the extensive method, at least.

Dollard: We might distinguish the first direct effect and various derivative effects. A derivative effect might be to stimulate the other powers to arm more vigorously.

Henderson: You might find that you could separate these investigations. A study, for example, of the display of power by the Germans and Italians in the Spanish civil war particularly from the standpoint of the derivative effects, might be quite fruitful.

Dollard: Project 10:3 on "Selection of Individuals as Targets" concerns the question, "What man would you have to hit to knock the country out, or what eight men, or what ten men?" It seems to me a problem of sociological analysis of the power structure.

Speier: The description of the project is unnecessarily narrow. The aim appears to be early surrender. It's very difficult to conceive that by the elimination of any number of individuals of the enemy elite you will effect surrender. You may effect disruption of certain activities on the part of the enemy, delay in making decisions, or disaffection in the population, or what have you.

Dollard: It is an extremely interesting problem to see if you can make a diagnosis of the power structure in a potential enemy country, and figure out some small list of people who are really critical in the situation.

Ridenour: How durable is this information about the people who are important targets because of their identity or their position in government?

Weaver: It is a good psychological and sociological question whether or to what extent it is durable or not.

Dollard: To put it in concrete terms, such an analysis done in America in 1941 would have been relatively durable, wouldn't it? It would have held true right through the war. I would say that would be even more true of a police state. It could change in the personal power structure much more dramatically, but rather less extensively or frequently than in a two-party country or Great Britain.

Weaver: Then you have the problem of what happens when it is unsuccessfully undertaken, as happened in Germany fairly close to the end of the war.

Hitch: I wondered if it isn't a continuing job for the Central Intelligence Group.

Ogburn: A good many of these projects center around this intelligence point, and could be classified under that.

Speier: It is possible that this project contains the germ of something else that may be of interest to researches interested in intelligence. Assuming that on the potential enemy side similar studies are being undertaken (which is a reasonable assumption), it might be possible to exaggerate the consequences for the purpose of deceiving the enemy. By way of illustration: We now know from documents that when President Roosevelt died, very high up in German elite -- that is, around Hitler and Goebbels -- it was generally assumed that this would have a major effect on the American war effort, and constituted a sort of a miracle in favor of Germany. Totalitarian regimes are, in my view, particularly likely to engage in this sort of wishful and unrealistic thinking. An exploitation of this weakness might be explored for purposes of deception of misleading the enemy or creating the wrong type of intelligence on their part. Such misleading information planted in, say, American newspapers picked up by enemy intelligence might be worth while considering. It's sort of black lies, but I don't think we should dismiss it unless we want to stifle our imagination. Maybe not much will come out of it, but it seems to me it should receive some consideration.

Dollard: Project 10:4 on "Effectiveness of A-Bombs on U.S. and Foreign Targets" has been rather broadly explored. Practically every agency in the Army and Navy, and various other groups like the State Department, have been doing studies on this problem.

Weaver: The Navy operates a research group that is spending some time on this. And I would imagine -- I don't know, I would imagine -- the Atomic Energy Commission is thinking about it in places.

Ogburn: Brodie published two papers along this line for the public library in Washington. He agreed there were some studies, but he wasn't very high in his opinion of their effectiveness.

Weaver: It is a very important problem, but I suspect that it is not necessary to amplify the effort that now exists.

Alchian: Concerning Project 10:5a on "German and Japanese Preparations", would such a project be of much use in the kind of war we might have in the future? I wonder if this isn't just a re-fight of the last war.

Wilks: There must be volumes of information that has been picked up by groups that went into these countries afterwards. I don't know how much digestion of that material is going on.

Dollard: I know of one small scale study on the German economy before and during the war which was made by a member of the American Army German Bombing Survey. Burton Klein is doing it at Harvard.

Ogburn: One angle of this -- the time required to prepare for war -- should be raised as a specific question somewhere along the line. The U.S. was precipitated into two world wars under rather favorable conditions, namely, that the enemy was being held while we could make preparations for war. That favorable condition is not likely to occur in the future. If the German and the Japanese experience were analyzed from the point of view of breaking down the preparation of war and making note of the time it took to get ready, it would be profitable. I was a guest of the American ambassador in Berlin in 1936, and I heard a report come in from the Dutch ambassador there of a meeting of German officials on the time they expected to be needed to prepare for war. It was very illuminating. In general it ran around eight years. Somewhere in the study of preparedness for war this pushing of the time factor should be studied.

Alchian: Wouldn't that depend on the kind of war you are anticipating fighting? I have a difficulty in conceiving of time of preparation for atomic war. Would we get the stock piles in advance, or do we consider the time at which we decide we are going to use war as a policy? I don't quite see what is meant by "preparing for war". How can we define it in terms of types of war which may be waged in the future?

Hitch: It is true that professional soldiers have a concept of being ready for a war. It's an absolute concept. They conceive of having a certain number of squadrons, a certain number of divisions, etc., trained up to a certain level of efficiency. But actually, it's a comparative thing. You're ready for war when you're in a better position than your opponent and likely to win it.

Henderson: When were we ready for war after Pearl Harbor? As I recall it, it was about eighteen months before we were ready for war. That is the time it took us to organize our institutions and our economy to put an army in the field. But even then, we were only ready for war in North Africa against a few enemy divisions

Henderson: and not ready for Europe. The length of time it takes to prepare for war after the war has been foisted on you may be different from the length of time it takes if you do it as a deliberate policy in peace-time and have to carefully handle the matter so that public opinion goes along with you. I think it's possible to learn quite a lot from a study of preparations in Europe.

Ogburn: When you think of the high degree of integration which exists in modern nations, you realize that all wars become total wars. The preparation for war, even on a military basis, would include the mobilization of certain economic institutions, and the preparation for war by mobilizing the economic institutions is quite an assignment. But I would particularly like to emphasize the point which Henderson made that it is two different propositions to mobilize in peace-time when you don't know when war is coming and to mobilize after a Pearl Harbor when everybody is practically unified. If it took us two years or so after Pearl Harbor it might take us a longer time without a Pearl Harbor.

Speier: It has been pointed out that the usefulness of this study would depend on the ability of the researchers to predict this analysis on certain assumptions as to the type of war that will be waged. In addition to that it may be necessary to take into account that the results derived from a study of German and Japanese preparations are not necessarily applicable to American society. The structure of your society, its division, and its institutions have a bearing on the applicability of certain methods. There is, of course, a trend toward obliterating these differences somewhat, particularly in wartime -- at least minimizing them in the interests of efficiency. But preparing for war in totalitarian countries is a different proposition than in a democratic country. If this study be undertaken, one should not approach it from a -- how shall I say it -- technocratic point of view, as though the results can be applied to any kinds of society without further qualifications.

Henderson: I quite agree with Speier on that, and I think this is precisely why you want to make a study. It's important not to limit the study to any particular type of war assumed in advance, because I don't think anybody's wise enough. One way of getting around the difficulty which Speier pointed out is to try not to limit it just to German and Japanese methods, but to any methods on which you can get some data and evaluation.

Dollard: There's a preliminary study out now -- a war studies committee on government in war time, tracing the history of the *ad hoc* measures that we took to organize our economy until we finally got the machinery we had at the end of the war. It's only a piece of the problem.

Hitch: It does seem that this project is too grandiose and too exhaustive. It seems to call for complete historical analysis of what the Germans and the Japanese did in the decade before the war started. I should think that we want to use this source material in investigating all sorts of problems that concern the Conference but I don't know that we ought to give high priority to this type of historical study.

Ogburn: From a research point of view, Hitch is quite right, but to look at it from the planner's point of view, it's another thing. I would say that this subject is almost worthy of a RAND conference on this topic alone. It would be a very large undertaking, but that it is big and important doesn't necessarily mean that we

Ogburn: shouldn't give it a priority.

Wilks: It's a question of its general applicability to America. It would be just a historical study on these two countries, who were aggressors. You've got an entirely different set-up there.

Henderson: Concerning Project 10:5b on "Selection for Military Service", from my experience in one section of the Army which was almost totally in the hands of civilians (and very competent civilians, if I may say it without seeming arbitary), the problem is not selection, it's assignment.

Wilks: Isn't there a lot of study and thinking about this problem now? At San Antonio, Texas, for example, the Air Forces have their personnel selection set up, and I know they've got a whole group working on just this problem of assignment right now. They've got all the cards there -- eight million of them.

Henderson: The only improvement that was made to my knowledge in the Army was to send teams out into the field to check classification against assignment and insist on assignment in accordance with classification. But classification by itself, by and large, was very good.

Wilks: Apart from the services, right now there is a committee of the Joint Research and Development Board which has been set up to coordinate and, presumably, initiate research along these very lines.

Alchian: Concerning Project 10:5c on "Analysis of Military Manpower Needs" -- "Would universal military training actually obstruct our preparation for war" strikes me as the most pregnant question there.

Dollard: There's almost no reference to the question as to what you train men for in the event of war. I feel very strongly about this. I went before the commission on universal military training and pleaded with them to make an analysis of what we were going to train for, and there was a good deal of sympathy in the commission with that viewpoint. But since they were already instructed as to what their report was to say, they were slightly handicapped in making it known. I think seriously that there is real danger (and I think many Army men would agree with this) that universal military training will obstruct our preparations for war, by diverting military attention and military commands to a relatively absurd program. And second, the program will collapse of its own weight unless we decide what we are going to train for before we start taking men. Whether that's RAND's project is a matter of judgment. It's somebody's project, and I think it's terribly important. Again, you have to set up certain assumptions as to what kind of a war you're going to have, but I think it's perfectly possible to set up some reasonable assumptions. As far as I know, and I read the Compton report quite carefully, there is no analysis there of even what contingents they are going to train for. You could set up a perfectly good case for an army composed 80% of policemen, firemen, and medical corpsmen, because that may well be what we need in the event of a war. And if that's true, the last place I would think of to train them would be in the Army. You shouldn't burden the Army with that sort of thing.

Wilks: I read the Compton report and the impression I got was that they were

Wilks: arguing mainly on grounds of sheer organization, that is, to have the manpower organized so you could mobilize it quickly. Otherwise everybody is scattered all over the country.

Dollard: But you have only a slight proportion in training at any time, so that argument isn't terribly good. Those who have graduated are in reserves scattered through the country. I think we may be heading into a really terrible mistake on universal military training. And I think it's the kind of problem that social scientists might contribute to.

Alchian: I think it is the most important project mentioned in this panel today so far. It apparently isn't being done elsewhere.

Hitch: What have the services done? Why are they supporting it? How did they persuade the administration to support it?

Dollard: I would guess that the reason the Army went on record for it was that it was the easiest way to maintain a large Army budget. I don't say that there was any selfish motive in that -- that's their profession. Army officers are only important if they have a lot of money and a large Army, as they discovered between the two wars. I am quite sure there are quite a few men in the Army who have serious doubts about universal military training, and the committee that was called in by the War Department to review the manpower needs took a very dim view of universal military training, because I happened to be on the commission when they reported. Among other things they questioned seriously was whether the Army had tried adequately or whole-heartedly to recruit men voluntarily to fill their needs.

Ridenour: Even though it turned out it were being studied elsewhere, it would be of great help to have competent social scientists from various disciplines give an expert opinion on this matter under non-service auspices.

Henderson: Yes I think so too. That's right.

Dollard: Project 10:5e on "Influence of Training on Morale" really includes two unrelated projects. The first is: Should small groups be trained together; the second, to what extent is morale influenced by indoctrination.

Henderson: As a very practical matter, you've got to add something to the question whether small groups should be trained together, if you are going to assume that they will remain together as a group. The Air Force crews were very carefully trained as groups and as soon as they arrived in the European area they were separated. This may have had some effect on the bombing accuracy.

Wilks: Weren't the commando groups trained together and didn't they operate together?

Alchian: Weren't these air crew groups separated because they did not operate well together?

Henderson: No; they were separated as a policy of breaking them up.

Dollard: On this second question, to what extent morale is influenced by indoctrina-

Dollard: tion, Stouffer is now reporting the result of his studies during the war. I think we will have some very good information on that problem. I don't see any immediate point in setting up new studies which would be infinitely more difficult to do now than they were during the war, where you had a good deal of control over your sample. Grinker and Spiegel published several very good volumes during the war and I've seen some Air Force psychiatric reports during the war which bore on this point. Unless you wanted to do very fundamental experimental studies, it's the sort of thing that would be very difficult to do during peace time. At least, I think it would be more profitable to analyze what we have now than to get more. Attention should be directed to analysis of existing materials.

Wilks: Project 10:5f on "Leadership in Small Military Groups" is probably one that could be done a little bit better in peace time.

Speier: I believe that some material was also available on that from OSS military records.

Dollard: There's a little, but not much. The best material was German. They gave it a great deal of attention early in the war. I happen to think that's important for this reason. We once made a small and modest study asking officers to rate the qualities which were most important in non-commissioned officers, i.e., in a squad or group leader. Then we asked the men to make a rating of qualities, and we gave them a series of seven qualities. The heirarchy of rating was exactly the reverse for the enlisted men and the officers. All the qualities prized by the officers were the ones least prized by the men. Now since the non-commissioned officer has to be a leader in critical situations, it's pretty important. The Tavistock Clinic in London experimented somewhat with another problem -- how to select junior officers. They had some really brilliant hypotheses which they were pretty well able to demonstrate, but which they weren't able to carry out because of the traditions of the Army. They set up a pilot scheme where the officer candidates were selected by the nominations of the men in the various companies. All officers made nominations also. They then plotted the choice of the men and the officers, and wherever they got an intersection they said "That's our man" -- and with very good results.

Wilks: This seems to be a project which has to be done in close cooperation with the Army, using Army men and Army groups.

Dollard: I have a hunch that it's the kind of project the Army might accept in peacetime.

Wilks: Yes, if you can get them to do a little experimental work.

Dollard: They'd have to agree to let you set up an experimental situation.

Alchian: The Air Force actually requested one during the war. They set up one but later closed the whole base down, so they couldn't do the study. But they started again when the war was ending. I don't know what results they got.

Dollard: It's a problem that puzzled and challenged Sam Stouffer for years. He's just intrigued with it. That's why I think the Harvard Department of Social Relations would be the ideal place to get it done.

Brodie: It is useful for another reason. That is the very obvious possibility that small groups will play a larger and larger role in future warfare. They played a pretty significant role a few times in this war.

Wilks: Project 10:6 on "Sabotage Against the U.S." is, I should think, an FBI job.

Ridenour: I wonder whether anyone would entertain the possibility of an experimental study of this point. The point is whether democracy can afford to do anything that will be effective against this danger. My personal prejudice is that the answer is no.

Henderson: I don't believe that it's necessary to experiment with it though. You might make a study of what went on in occupied Europe.

Ridenour: The dangers in considering this matter at all are great. Many people may feel it necessary to take the most absurd measures to guard against such possibilities, and it's quite restrictive, unpleasant, and expensive.

Ogburn: I wonder whether this project might not be tied to military intelligence. I don't see any projects here that have to do with development of the intelligence service, which is a much bigger problem than this. I presume this would be taken care of by an intelligence service. If it is a matter for the FBI it would call for a considerable enlargement of its scope. I would like to get into the record here the importance of military intelligence.

Hanson Baldwin puts that as the first of our military needs at the present time. I certainly think it should rank very high. In view of the new nature of warfare, which involves attacks on civilians, hiding of bombs and various devices of this kind, it would seem to me the intelligence aspect of warfare has a certain relationship to the social sciences. For instance, one feature of intelligence in the old picture of war was ferreting out military secrets and finding blueprints, but in the new one intelligence spreads out to something that resembles what our ambassadors or ministers might do. It revolves on reporting public opinion, analyzing national character, etc. This project seems to need to be related to military intelligence. This conference ought to get into the record somewhere its awareness of the importance of military intelligence.

Wilks: In connection with Project 10:6, the records of FBI would be almost indispensable to any kind of an effective study of the problem. They've got records they kept through both wars by cities by states. I can't conceive of making a useful study of this without working with FBI, except to try some of these experiments mentioned by Ridenour. Maybe FBI has done that.

Dollard: The Central Intelligence Group is pretty actively recruiting men for the intelligence service. Judging by what I have heard in our office they are after a very high level man who could do what Ogburn proposes.

Ridenour: Project 10:6a on "Sabotage as Counter Attack" is sufficiently different from 10:6 to be given some consideration. I don't know enough to know what values would come out of the study of the applicability of methods of sabotage. But it might be that someone who had related intelligence experience during the last war

Ridenour: (or ten such people, as suggested here) could make a study that would have more or less definitive results as to whether sabotage is worthwhile in comparison with other means of expending the same effort.

Speier: The question is the military worth of sabotage. It seems that the person who drafted it thought primarily of the disruptive effect sabotage might have on the morale of the enemy population. But of course, one could easily think of a number of other objectives of sabotage, such as disruption of an industrial process. I would suggest, if the panel considers this project important, that it be enlarged along these lines.

Ridenour: Can we relate this to Project 10:3? In both cases aren't you talking about the analysis of the class and power structure in the enemy country?

Ridenour: The same argument raised against 10:3 -- that it was not properly a subject for this panel -- would hold against this one. On the other hand, I would like to say again I think the problems raised in both the projects are very important and something the social science techniques could deal with.

Speier: If you really want to have reliable information on the susceptibility of the Russian anti-Soviets, you would have to get data in an extremely dangerous way. You could not rely on the interviews of recent ex-Russians. Nor could you rely on data drawn from French and other underground movements during the last war. It's a matter for top-secret intelligence.

Dollard: I understand that quite a few former employees of the Russian wartime mission did not return to Russia, but are in this country. I would think there was a positive selection in that group as regards political allegiance to the Kremlin. Would that not be a group of witnesses who could contribute something?

Speier: I should be inclined to think that they would contribute something. It's a source that would have to be tapped, but I would not think it is the most important source, because time has passed and the information would not be as reliable as what you could get through other sources. Isn't there a great danger that the information that you would obtain in this way might be emotionally colored? They would like to be useful to the American agency interested in it, and you would get a distortion of results. Still you would want to interview them, but I don't think you can base your study on it.

Weaver: I have just been reading Allen D——'s book on Germany's underground. I don't know whether its fair to make an analogy, but certainly D—— takes the position that there were individuals whom he could name who were anxious to participate in a campaign of sabotage. He also makes some implicit generalizations about categories of people in Germany who might have been mobilized for sabotage. I think it is worth trying to make both of those efforts, however meagre your evidence is, and make them with the full knowledge of how inadequate your evidence is.

Ogburn: The Russians made a good deal of German prisoners captured in the last war for information purposes.

Weaver: I'm not speaking now to the sabotage point, I'm speaking to what I think is

Weaver: a basic point -- what analysis can you make of the social structure of a potential enemy country which would help you determine effective methods of psychological and undercover warfare. I can think of several people who would have ideas about it. I don't know how many of you saw Percy Corbet's memorandum on his visit to Russia, published by the Yale Institute of International Studies. I thought it exceedingly perceptive. Lasswell went through all the countries just outside the curtain about a year ago for the State Department, and I am sure he would have some ideas. Alexander Leigh did a somewhat comparable job on the Japanese during the war, and he has a lot of ideas. It seems to me this is a little bit in the same category as the question of the theory and practice of military security, in that we would all agree there is an important problem and important work to be done, but that you're condemned to do it in vacuo and ineffectively unless the thing starts from the inside of the responsible service. It doesn't seem to me that this could be done effectively unless it had the really full cooperation of the Central Intelligence Group and full access to records of the OSS. In the nature of things it would not have that access unless the primary interest in the problem arose there. If you could influence them to want to have such a study made, that would be fine. I am a little skeptical about attempting to try to button that kind of a project onto them externally. I think it would be a patch pocket and nobody would ever reach into it.

Wilks: That's my own feeling. I can't see RAND doing it as a separate project unless it can get it requested by Army or Navy Intelligence.

Dollard: The reason I am keen about it is that, quite independently of RAND, we have formulated the same problem and have made very tentative overtures to the State Department and Central Intelligence and found a vast amount of interest. But I still disagree with the majority opinion. I think it's exactly the sort of thing that RAND should do through these other agencies.

Weaver: Concerning Project 10:7 on "Analysis of Past Wars (End Position)," most of you know that there have been some numerical theories of competitive struggles, for instance, the Lanchaster theory modified and extended in this last war. There is some interest to see whether these numerical theories bear any interesting relationships to the facts of the actual war, whether it is possible from a historical examination of past wars to get any important common elements in the victory-defeat relationship. I am completely skeptical about the second matter myself, because ever since I read Henry Adams' "Decline of the Democratic Dogma" I have come to the conclusion that there is no such thing as a theory of history. I think you can possibly find out why some wars have been won in the past, but I am completely skeptical of being able to find out from that anything useful or significant as to what will win wars in the future. On the other hand, I think you can make a useful examination of the extent to which the numerical theories of combat apply. It seems extraordinarily difficult to find past battles or wars on which there is enough good numerical data to make any check at all. So on the study of end-positions of past wars I am completely skeptical, because I have no confidence in theories of history.

Alchian: There might be some dispute about the theory of history, but I suspect the conclusions would be right even though that particular hypothesis is wrong.

Wilks: I take it there's no enthusiasm for this project except for the mathematical part.

Weaver: I let a little too sour a note creep into my voice if you interpret me as saying there is no enthusiasm. I am curious about this. I would like to have somebody convince me that I'm wrong about learning history. Maybe it's just because I don't like to study history that I don't think we learn much from history. I don't think it's very practical to study from the point of view of our interest.

Hitch: Concerning Projects 10:8 and 10:8a, could we persuade the services to ask RAND to set up a team of people from different disciplines to examine the various methods of target analysis that are now being used in the services with a view to improving them, instead of RAND attempting to make target analyses itself?

Henderson: That's a very good suggestion.

Weaver: This is a very important topic and is being handled exponentially less well as we retreat from victory. It is almost impossible to maintain the morale and the quality of personnel in that kind of an enterprise under Civil Service in Washington. The Joint Target Board badly needs help from the outside. Some of the directions in which it needs it worst are along the lines of how they can establish contact with really competent knowledge and experience in economic and political fields. The mere mechanical techniques of target analysis were reasonably well developed during the war. If the services could be persuaded (and I don't think it would take much persuasion) to back a project on the part of RAND to furnish the structure for such an operation and the general planning for it, it would be a very significant thing to do. I don't think it is being done well now.

Alchian: If you interpret target analysis in a broad enough sense to include factories, civilians, methods of bombing, and methods of affecting their fighting, you might have the whole social sciences in. This project might be one of the doors through which the social sciences can come into this thing. Is that a correct conclusion to draw?

Weaver: In the first instance one would talk about actual physical destruction and therefore actual economic rating of the target. But there is a whole sequence of further definitions of attack weapons down through political and social and psychological factors, and then your ratings change.

Hitch: I don't think it should be proposed that RAND should build up a staff to carry out target analysis on a continuing basis. But RAND might be concerned with the methods that are being used, and with establishing relations between the groups and services that are doing this and the outside experts who are advising them.

Ridenour: I am afraid I may misrepresent the actual feeling of the panel with respect to the sabotage project unless we say another word or two about it. I have remembered some fairly novel suggestions for possible forms of sabotage that some of the RAND people tossed around. Some of these seemed to offer the possibility of making experimental trials that would determine their effectiveness. We rather ignored that particular angle of it. A possible result of the experiments is that, if there seemed to exist a practical method of doing such sabotage, one could devise monitor schemes which give some possibility of warning against it. They might not be too elaborate, expensive or troublesome, and they might save the lives of some half-million people at some time.

Dollard: Ridenour remarked that he thought any attempts to study sabotage would bring us squarely up against the question whether you can afford to be afraid of sabotage in a democratic country. I am very curious as to what his own answer is.

Ridenour: I see no harm in taking some simple precautions gadgetwise. The sort of thing I am afraid of is large-scale restriction on the movements and activities of individuals. These novel types of sabotage which you might investigate would probably lend themselves to the gadget type of defense.

Dollard: Some photo-electric cell monitoring of the New York City water supply, for example, is a trivial problem. It wouldn't cost anything to speak of, or be any trouble. Throwing guards around the reservoir is different. So I would withdraw my objection on that basis.

Ogburn: I have an idea. I don't know whether I can formulate it very well. It has to do with technological development and future wars, particularly as it would effect social and institutional relationships. I suppose after World War II, where it was said that mass production and the laboratory won the war, we all think technology is being pursued adequately for purposes of warfare, particularly if we should include a Science Foundation. It may very well be that is the case, although I think the history of military experience shows that there are many unfortunate victims of wars who did not adopt scientific and inventional developments as soon as they might have. That applies also to the United States. There turn out to be social aspects of these technological and inventional developments that might be explored and examined. I am particularly impressed with a phenonenon which recurs again and again in social science -- a sort of resistance to the adoption of an invention or adjustment to some technological development. Most of us think that technological developments produce tools to put in our hands and we are very eager to take them over, whereas many times we are resistant to their development. There might be included in this section some sort of reconnaissance survey on the technological outlook, with reference to how it affects the institutional, social, and economic relationship to war. You can see on a moment's notice that the adjustment toward a bombing by relocation of population or industries runs into very many difficulties. It seems to me practically an assured fact that the adjustment will not be made; the obstacles are so many and so great. This does not mean that this method should not be considered. What I am saying is that there are these social aspects of science and technology, particularly in a war, which calls for the cooperation of social and economic institutions. There is a segment of the social relationship which I fear may escape our notice in our concentration on more dramatic matters.

Dollard: Ogburn is too modest. He has not only thought but written a great deal about this problem at various times. It might be worth RAND's while to look back over some of those published papers and also try to get away from him some of his unpublished papers. Some of them are pretty provocative on the subject.

COMMITTEE IV
INTELLIGENCE AND MILITARY AFFAIRS

W. Weaver (Chairman)

O. Helmer	W.F. Ogburn
L.J. Henderson Jr.	L. Ridenour
A. Kaplan	H. Speier
H.D. Lasswell	J.D. Williams
D. Marquis	S.S. Wilks

Weaver: Panel 9 on Intelligence and Information was theoretically concerned both with the gathering and the dissemination of information. The panel spent most of its time and energy on the former. They spent most time and energy talking about content analysis and what one might call a systematic theory of the interview.

The project on the social science use of an accurate map reminded me of two things. One was that (perhaps because I had fallen under the hypnotic spell of Isaiah Bowman) I supposed that the millions of maps had to a considerable extent solved his problem, and second, that I had learned that some military operations in this last war had gone completely wrong not because of errors in navigation devices, but because the maps themselves were fundamentally off about that amount. That leaves me on two sides of the fence, and I don't know whether the mapping problem remains important.

Panel 10 dealt even more specifically with topics which arise in military affairs or in military theory but concerning which one may ask whether the various branches of the social science can help. The projects viewed by Panel 10 come usefully under four headings. (1) A considerable number of projects come under the general heading of Analytic Study of Warfare, the more or less all-inclusive topic being Project 10:1a on Military Worth. Underneath that are a series of sub-topics of Military Worth -- e.g., Project 10:1b on the Perfect Weapon concerns simply the weapon which maximizes Military Worth. Project 10:3 on Individual Persons as Targets is obviously one particular type of target analysis -- a kind of target analysis that is connected very intimately with the social sciences and which depends upon a knowledge of the power structure of the society, rather than upon physical questions of terminal ballistics. But it is nevertheless a sub-topic of Military Worth. So is the project on the effectiveness of A-bombs. Then there was a more general topic of logical analysis of military concepts and studies of previous wars, but all of those are different types of analytical studies of war. (2) Secondly, Panel 10 talked quite a little about the general problem of mobilization and utilization of manpower; six projects come under that general heading: Analysis of World War II Procedures, Methods of Selection for Military Service, Manpower Needs in Modern War, Optimum Training for Military Needs, the Influence of Training on Subsequent Morale, and Training for Small-Group Leadership. (3) Thirdly, there was a little discussion on preparation for war. The emphasis was chiefly on the point that two things make the preparation for war now

Weaver: quite different, for us at any rate, that it has ever been in the past. One is the time scale of modern war, and the other is the extremely small probability that anybody else will hold the fort for us next time until we take the time to prepare. Those two things would appear to make it absolutely essential that we have peacetime preparation for war as we have never had it before, if we do indeed decide to be prepared for war at all. That poses a set of extremely complicated, and for the most part rather new, problems for a democracy. It has been done relatively easily in a totalitarian government, but requires a whole set of ruled activities in a democracy. Many of these problems reach very deeply into the social sciences. We didn't spend as much time talking about this topic as we should have. (4) Panel 10 also spoke briefly about the problem of sabotage, both sabotage directed against us and sabotage which we ourselves might conceivably use.

Lasswell: Perhaps a disproportionate amount of attention was given to the problems which are connected with two aspects of content analysis -- one, the possible articulation of intelligence from content analysis with the entire intelligence process and the other, the testing of various technical procedures involved in the operation. This left uncovered any very general consideration of the over-all problem of intelligence organization, although it was stressed that there has been a minimum of investigation conducted by people qualified in the social and psychological sciences of the comparative history and analysis of the operations of intelligence agencies, and that there would be some advantage in considering to what extent research could usefully be directed into all of the various problems involved in the operation of an adequate intelligence agency.

Weaver: To what extent have the people interested in content analysis made studies where all aspects of the particular field of knowledge were actually accessible but where they artifically shut themselves off from certain parts so they had a completely controlled experiment? For example, how much could one find out about the physics that was done in the United States in a given year by looking at one out of every twelve copies of the Physical Review? What was done is all known and available; yet, it is perfectly possible to isolate somebody from 11 out of 12 copies of the Physical Review. Still more interesting, how much can be found out about what happened in the United States in physics in a given year by studying, let us say, three out of twelve copies of Popular Science Monthly, or perhaps by studying the pulp magazines, where the time situation would be different, both positively and negatively? Have studies of that controlled sort been made in the field of content analysis?

Lasswell: I don't think that I could cite studies that have been satisfactorily controlled along that line. There have been a good many investigations which have established partially verified relationships of this kind.

Speier: To my knowledge controlled studies have not been made, but I think that the general uses to which content analysis has been put, particularly during the war, did not include the attempt to solve problems of the kind raised by Weaver. If the government agency were interested during the war in what German physicists were doing, they would not have turned to a propaganda analyst to receive an answer. They would have turned to a physicist who would also have made a content analysis of all the material available; but that would not be the type of content analysis which usually is referred to by this term.

Weaver: I would suppose that there were techniques which that physicist would not know. The idea seems so attractive to me that, just as you can with certainty state that a person cannot conceal the fundamental characteristics of his own handwriting however much he attempts to, so you cannot succeed in concealing the fundamental intellectual activities of the nation, however much you attempt to. I think they just leak out, so all you have to do is think real hard about the initially curious and uncorrelated form in which they leak out. For instance, if you had simply graphed the frequency with which the newspapers in this country use the word "cyclotron", you would have found that there was a moment in history at which that frequency dropped precipitously to zero and stayed at zero ever since. That absorption band in terminology was, as a matter of fact, just about as significant a thing as could possibly have happened. To my judgment, it indicates a stupid theory of military security. Anybody with just a modest amount of common sense would have discovered by absence exactly all of the things that Mr. Groves and his cohorts were trying to keep them from discovering. That may be over-simplified, but I can't help but be very enthusiastic about the possibilities in this field for intelligence.

Speier: It is indeed a fact that significant omissions or the significant suddenness of disappearance of certain key words in mass communications was made the subject of study, on and off, in content analyses carried on by governmental agencies. The difficulty there is, of course, that the content analyst will not spot a significant omission or a significant suddenness of disappearance of certain key words unless he is informed by an expert what to look for. Among the content analysts employed to my knowledge during the war there wasn't a single physicist.

Weaver: That seems to me deplorable. Before you came to the last remark I was simply going to say, "Well, Dr. Speier, you are simply explaining why both you and Dr. Ridenour are sitting on that side of the table." In other words, this conference, I would say, is better than some of the activities in this field during the war.

Lasswell: That is emphatically correct, and I would like to underline this point. One of the potentialities of this procedure can only be utilized if the problem is satisfactorily met of arranging for the people who are specialized and sensitive in the relevant field of knowledge to interact with those specialized in the description of the flow of communication. What is required is a solution of the administrative questions which are involved in getting people of those sensitivities into effective working relationships. The possibilities of making use of the content analysis procedures along the lines you talked about are very promising indeed. I share all of the optimism implied in your remark. Any investigation of the use of content analysis in the past would need to be conducted with a view to indicating the opportunities that were missed through lack of this type of team play. The technical problems of validation of content analysis procedures are of relatively little interest. They are comparatively simple. But the problem of getting ideas as to what's worth looking for and of how to interpret the indications requires the most expert correlation of expertly trained and selected talents.

Speier: I would like to add one illustration of the problems we have just been discussing. I remember one instance during the war when in a content analysis group in the Federal Communication Commission a little study was set up on the content and frequency of mention of certain medical devices, facts dealing with healing of wounds, etc., in German mass communications, particularly radio and newspapers. The study was

Speier: originally set up in this group because we felt that such a study might throw light on certain worries or apprehensions on the part of the policy makers concerned with German morale. We hoped the studies would throw light on the worries German propagandists might have about anxieties in the population at large about wounds and accidents and the casualties in the war. We came out with a study of some length, and found to our surprise that the interest the study evoked was not confined to people interested in German morale, but extended to those interested in the development of German medicine during the war. So the study was forwarded to these people, who, we of course assumed, would have much better sources of information than the newspapers and the radio. We never heard from them again; we didn't hear from them that they were interested in medical information rather than in morale studies in relation to medical problems. Nor did we hear about the intelligence value of the study on the development of medicine. Finally, we didn't hear anything from them about any particular types of information we should in the future be looking for. There was simply no correlation between the content analysis for studying morale and content analysis in the less technical sense for the purpose of obtaining technical information on a particular discipline like medicine.

Ogburn: As I understand it, intelligence in the past has often been thought of as closely related to espionage -- particular things like blueprints or photographs are supposed to be sought. There is another type of information that may be illustrated by some such information as we might suppose the Germans may have wanted at one time. It would have been highly desirable for the Germans to know to what extent the British, after Germany's first successes in France and Belgium, were likely to put up a strong resistance. Another non-gadget type of information which the Germans in both world wars would like to have known was the probability that the United States would be drawn into the war. It does not seem to me that you get these types of information by content analysis nor by the interview. The question for this general type of information is -- What is the technique by which you attain it? That is a topic which we want to consider.

Weaver: There are inter-relationships which you cannot conceal in this interrelated world. Certain long-range policies and tendencies of a government would be discernible in what happened to their social and economical institutions. A study of the effect of war and of the preparation for war upon social and economic institutions would itself give you a basis for discovering certain long-range effects. As to the short-range effects, of course, those of us who do not understand such matters are a little worried by what appears to be a rather wide swing of public opinion. I am thinking, for example, of Mr. Truman's popularity over the last 12 months; it is rather surprising to me the way it has moved about. Further, have any of the people who have been concerned with the theory of public opinion sampling studied the problem of the optimum sampling procedure for an individual? I suppose most of their theory has been the theory of public opinion sampling for a rather widespread organization. But it is not completely stupid to say, if we're going to have one man in Chicago, what is the best set of things for him to do? How many taxi drivers does he talk to, how many trustees of the University of Chicago, and what is the weighting function applied? That may really be a sensible question with respect to public opinion sampling, although obviously not the one the organizations are interested in.

Ridenour: I'd like to second the suggestion just made and make another. Intelligence

Ridenour: is usually spoken of in the sense of gathering information about someone else's activities; the other side of the coin is trying to conceal or mislead someone else with regard to your own activities. There is a third meaning of intelligence, which in a sense is an aspect of the first, although it isn't usually recognized as such. When one is running a large and complicated effort such as a government or a war, inevitably it has many branches and departments, with not enough connections in its nervous system to make sure that everything which is known by the right hand is in fact known by the left. Examples of this are not very far to seek. I am sure they stand in everyone's wartime experience. Shouldn't intelligence also concern itself with data which originates entirely within one organization? How can the government make just as sure as it can that it doesn't have wastefully parallel efforts, that it doesn't have valuable results sitting in Washington that could be very useful in Chicago, or that the National Bureau of Standards, say, isn't doing something which would be of enormous value to some entirely different and at first sight unrelated agency? The problem is made more difficult by the superstition that you assist other nations unless you make a positive effort to reduce such coordination. And is there any way in which the following problem could be studied: Is it more effective to conceal various classes of information than to make it public and disseminate it in a way that's been thought out? Here's an illustration of the problem. The U.S. Army had the very best anti-aircraft equipment in the last war, as is admitted by all on the basis of the record. One reason that it had this equipment sooner and that it was possibly more effective than it might otherwise have been, was that the design of the radically new and very effective electrical computer was very carefully integrated from the start with the development of radar which was meant to supply positional information to the input of the computer. This sounds like an elementary bit of coordination which could be done entirely within the bounds of military secrecy. In point of fact, there was no thought of doing this on the part of an official. The two devices were integrated because, so far as I know, two men, one connected with each development, happened to meet on the piazza of a colonel's house. Had it not been for this chance meeting and the fact that these two men fell into a conversation which led them to make this discovery (which was quite illegal for them to make, of course), I am personally convinced that it would have cost us six to eight months' use, which really means all of the effective use of this very powerful weapon.

Lasswell: The problem being discussed is one of the problems which has occupied some of the other panels extensively. I am referring concretely to the project on the conduct of foreign policy by democratic governments, where one of the major problems is the degree of secrecy and non-secrecy. Who can effectively know what with certain consequences for security and for other specified criteria? The second relevant project is the analysis of the relationship between civilian and military judgment of what constitute valid claims of military necessity justifying non-disclosure.

Wilks: In Panel 10, concerning the first project, on the Logical Analysis of Military Concepts, our initial feeling was that it was very general and diffuse and broad, as broad as the whole military system itself.

Helmer: In many of the physical sciences it has proved extremely helpful to go through a process of logical analysis of the basic concepts. Presumably the intention behind this proposal was to apply similar techniques to military science in

Helmer: order to enhance the understanding of the theories involved and also to find out what possible theoretical gaps there might be in this theory.

Wilks: In pursuing the study of military worth you normally run into the problem of analyzing a military concept; so this is part of the latter project. The Panel felt definitely that Military Worth and Perfect Weapons were just the sort of projects RAND ought to be concerned with, and the Panel was very much in favor of seeing these studies continued. Concerning Project 10:2 on Peacefare Use of Military Weapons, the panel endorsed the general idea of trying to find the effect of the use of military power in peacetime for political reasons. Concerning Projects 10:2 and 10:6a on Sabotage, the feeling was that these would be useful things for somebody to do. If some kind of needling could be done to get the FBI to undertake these or to request that some studies of this sort could be made, that would be all to the good. Concerning Project 10:4 on Effectiveness of A-Bombs on U.S. and Foreign Targets, the feeling was that a good many studies on this topic by military groups are under way, and that an intensive study of the problem would involve a lot of duplication. On Project 10:5a on German and Japanese Preparedness, the general reaction was that it was a historical study of two dictatorships, essentially aggressors, and that the real question was to what extent any results from a study of this kind would be applicable to the U.S. We weren't at all sure of the value of such a study. The panel opinion of Project 10:5b on Methods of Selection For Military Service is that it is probably being very thoroughly handled. The Army and Navy both have very thorough personnel establishments and are doing a considerable amount of work on this. There was one aspect which was emphasized, however, in the belief that it may not be considered by the groups in the Army and Navy -- namely, the assignment of personnel once they are in the service. That is very important, and some effort should be made to find out to what extent this problem is being dealt with by the various military groups. Concerning Projects 10:5c and 10:5d on Military Manpower Needs and Training of Military Personnel, one of the members of the panel was quite strong for seeing some kind of a study made on what you train young men for if there is such a thing as universal military training. Dollard's point was that, as far as he knew, no systematic study had even been made of this problem. Maybe in a future war the conventional type of training would be completely obsolete. It was agreed by the panel that it is important to have some study set up concerned with what kind of training the men get and how the training is distributed. On Project 10:5e on Influence of Training on Morale, the feeling was that studies on data from World War II would be fine, but that any attempt to study this problem on peacetime data would be likely to be misleading. The Panel recognized that a number of studies along this line have already been made. Some psychologists and psychiatrists are also making studies similar to Stouffer's; the feeling was that other studies based on World War II data would be desirable. On 10:5f on Military Leadership in Small Groups, the position was that there is probably something to be done.

Marquis: The point was made that it was the type of problem which could be done in peacetime, and that it might conceivably be a very important problem in another war, where you may have a great many small units operating independently, or where you might at least always have to face this contingency.

Henderson: This particular problem is especially approximate to the Army Air Forces. An illustration that indicates the kind of situation which raises this

Henderson: question was given in the Panel. We made very simple studies during the war which asked officers and enlisted men to rate non-commissioned officers on the traits which were most necessary or desirable in a group leader. The ratings of the officers were exactly the opposite of the ratings of the men. Unfortunately, it's the officers that pick the group leaders, obviously on a basis which has nothing to do with the functioning of the group.

Williams: On the study of U.S. and USSR targets, the thought that was in our minds in Santa Monica was that there are groups presumably studying the effect of A-bombs on Russian cities, and presumably also groups doing a little work about A-bombs on U.S. cities. Here Ridenour's point is relevant: are these groups together, or are they bringing their results together in anyway? There's the point that so many of U.S. cities are located on or adjacent to large bodies of water. A-bombs may be extremely effective against U.S. cities because of the radioactive mist possibility.

Henderson: The major work on both those specific problems is being done in one place under one direction. I say this without reference to whether it is adequate work. It is very important to consider whether it is adequate work or not, and this was the invention of the result reached by the Panel with respect to Projects 10:8 and 10:8a, where it was pointed out that an analysis by an outside agency of the methods being used on the general problem of target analysis and vulnerability was very important.

Weaver: The really important things to do for the military are the cases in which (a) they don't know what they want and (b) want something wrong. Actually those cases are fairly numerous. Therefore as large a degree of careful planning should be put into a field which is of explicit service to the military as to any other field. There have been mentioned two or three instances here this afternoon in which they don't know what they want or want something wrong. For example, there is a considerable suspicion or fear that the military people have been enthusiastic about universal training partly to perpetuate their profession and institution. Therefore they have not been quite as imaginatively concerned as Dollard and all the rest of us would like to see with the problem of what you really need people for. The problem is put vividly in the statement Dollard made the other night: Is it not very likely true that what we need to train is a good many firemen and a good many other people trained in first-aid and other aspects of disaster technique? That is one of the major things that should be done in this so-called universal military training, and really very little of squads-right-and-left. It is still highly probable there will be quite a little squads-right-and-squads-left in this program, and that isn't hard to understand. If you spend all your life squads-right and squads-left, you kind of want to impose it on the next fellow. It's like the fraternity initiation theory. That is a very important thing for us to watch for. There are several other points of that same general character. In thinking of the over-all aspect of this Panel's curiosity, we should think in a quite rugged way about the things the military ought to want and the things they may want mistakenly.

Kaplan: In relation to Project 10:2 on Peacefare Use of Military Weapons, did the Panel consider the possibility of any other peacefare use apart from the display or publicity given to the weapon? In the past, instruments of violence have been thought

Kaplan: of as serviceable in two possible directions: either by the actual imposition of violence, or by the threat of violence. Presumably display is effective in terms of this kind of threat. There are various parts of the military resources and installations, not necessarily weapons in the narrowest sense of that term, which do or could have certain peacetime uses. If, for example, there were established a system of air traffic control, it's plain that such a system would be not only of military service, but could also be exploited to the advantage of commercial airline operation. That is a peace*time* use, but not a peace*fare* use. Did we sufficiently consider the possibilities of making use of military resources in a way which would contribute to the fundamental objectives of the military, namely, to enhance the security of this country?

Weaver: There are two directions in which that has been discussed in other RAND conferences: First, through extensive peacetime application you can sometimes obtain a social and practical justification from a budgetary point of view, which otherwise would never be approved. You might be able to maintain a number of altered aircraft if they were actually in peacetime use for freight purposes, express, meteorology, perhaps even as relay points for television programs. You could get a budgetary basis for this expenditure which you could not otherwise get. Secondly, because people always get disinterested in a business which is not active, the only way to keep military people alerted and constantly in practice is to have them doing something steadily which they know is useful; and that means to have them involved in a peacetime use. The great radar chain could be regularly used, for example, in peacetime communications.

Kaplan: I had not so much in mind simply peacetime uses of this kind but specifically peace*fare* uses, that is, the possibility of exploiting the political value that might lie in military resources. In one of the panels in the political committee we were very much concerned with the information policy of the U.S. and how our goals and values could be presented to various regions of the world population in the most favorable way. It might be fruitful at some time in the course of considering this whole area of social science research to consider the possibility of the military being directly interested in such applications, not merely from the viewpoint of thereby obtaining certain budgetary assignments, but also from the propaganda value externally and from the political value internally in support of the general end -- national security -- to which the military is instrumental.

Ridenour: You are saying that in addition to the use of these weapons as an implied threat, one should seek also for uses which are helpful politically?

Kaplan: Yes, and in manners not limited to display.

Henderson: Perhaps crop-dusting or using transport aircraft in an emergency to transport large quantities of food or medical supplies, etc., to foreign countries are examples.

Kaplan: The last is certainly along the lines I had in mind.

Ogburn: I used an expression in one of the conferences which seems to me picturesque at least, the conception of "interchangeable parts" which is used in weapon making

Ogburn: and industry. If you conceive of two orders, the peacetime order of society and the war state, and you've got to make a transition from one to the other, then the expansion of interchangeable parts seems to be a useful concept in line with what Kaplan is talking about. In a democratic country we have a real problem in preparing for war in peacetime; it doesn't seem to me this conference has quite recognized that point. That is one of the reasons why I wouldn't consider the project on German and Japanese preparedness solely a historical study without any relevance, because they did prepare for war in peacetime. Also, I think we have magnified the differences between the social order in Japan and Germany on the one hand and in the democratic countries on the other. I could point out a great many similarities between them. I wouldn't think anyone would study the German and Japanese experience with the intent of mechanically taking up something which they did. But a lot of wisdom could be gained from studying that experience if you thought of an adjustment and adaptation to it.

Weaver: I might suggest the possibility of studying the theory of expansion of organization. There is nothing more important to the military when war comes than the theory and practice whereby they expand. The difference in the essential characters of the Army, Navy and the Air Force can be largely explained in terms of their expansion problem. As far as I know, the only thing which the military have done with this problem is to discover the French word 'cadre' and to say "We pick a cadre out of here and transplant it over there, build it up, and then transplant another cadre." Having discovered this word, they are under the impression that they have solved the problem of expansion. The problem is a terrific one, particularly for the Army; it must be susceptible of analysis. That is an important part of preparedness, because to the extent you can improve your theory and practice of expansion you can increase the ratio between your fully mobilized strength in war and your necessary peacetime strength.

Speier: It is a very important project, as apparently we all feel. I would like to add by way of supplementing Ogburn that the question of preparation for any possible future war is not merely a matter of expansion of organizations and preparation of necessary facilities and instruments. A very important aspect is the psychological preparation for war, in view of the fact that the speed with which this kind of preparation will have to be made will be practically infinite. There will presumably not be much time from the outbreak of war to the moment when severe deprivations are inflicted on the civilian population in which to prepare them for such deprivation.

Henderson: I should like to speak briefly about something which is in a sense a corollary to what Speier just brought out. The military are very much concerned about the problem, and they may be overanxious to take active steps in a direction which they seem to feel is important. This is the famous problem of declaration of war and how the U.S. can behave in an actual or immediately impending military crisis. The military seems to be thinking seriously about taking steps to have legislative changes made. Should they be successful, they might possibly bring about a situation where it would be possible for a military commander to deliver an attack upon what he would consider an aggressor (or a likely aggressor in a matter of hours) without consulting the President, public opinion, Congress, or anybody else. There is a feeling on the part of some in the military that this would be a desirable situation. Their comment is, "If we don't do this, we have a

Henderson: feeling that at some time in the future our entire force may be destroyed within the first few hours, and the war be lost." Wouldn't it be useful to make a study of this problem? For example, as Ridenour has pointed out, is the risk of war greater if there are two potential competitors each of whom has the ability to strike first without waiting for the other to launch an actual attack? We might all agree that should there be an actual physical act of aggression on the part of the enemy, it may be vitally important for the military commanders to act at once -- there may have been an interruption of communication. It is very difficult to foresee all the implications of this. How far should we go in attempting to change the present status? Is it possible that we may go too far and act in such a way that the rest of the world considers us the aggressor? This is a serious problem; it might be possible for some people to do some useful thinking in this direction.

METHODS, ORGANIZATION AND PLANNING OF RESEARCH

PANEL XI
RESEARCH METHODS

O. Helmer (Chairman)

C.I. Barnard
L.J. Henderson Jr.
A. Kaplan
E. Kris
D. Marquis
F. Mosteller
S. Stouffer
W. Weaver

Helmer: The first project is on the Composition of Research Teams -- the composition which most favors achievement in a given time or greatest ultimate achievement, taking into account the type of problem, the number of persons in the team, the skills present or demanded, and the major differences in personality characteristics.

Mosteller: We have recently done two small pilot studies. One was at the Department of Social Relations at Harvard, with graduate students working on small tasks. We did a second pilot study at the RAND Project this summer lasting about four days, again problem solving by several groups of various sizes -- one, two, three, and four. The main indications of the work were that sometimes the problems about compositions of groups in problem solving can be studied by using regular experimental techniques. It wouldn't take a great deal of ingenuity, but it would take a great deal of hard work to pursue the matter a good deal further. Some information could certainly come out at the end of a couple of years. If the Harvard group did it, they would not restrict themselves to this problem nor would they necessarily take the problem as now formulated.

Weaver: War being an experiment for which we pay such a price in dollars and blood, it's always seemed to me that we ought to treat it rationally as an experiment after it has been forced upon us, and try to get out of it all the useful conclusions for the future that we can. There was conducted during the war a not wholly novel, but in terms of its intensity and scope a rather new type of experiment in this field, namely, operations research. I am myself convinced that something rather new in terms of scientific methodology is involved in the organization of the mixed team approach to operational research problems. I had hoped very much that some group would make a really careful and critical study of the success of the various operations research groups. They were so widely disparate in their organization and in their administration. Some of them had exceedingly strong central teams on which they could depend; others, such as in our own Air Forces, had absolutely nobody at home on whom they could depend except a couple of Harvard lawyers. I would like to see an analysis; it seems to me there is a great deal to be learned in terms of the development of scientific methodology from that study. Raw data for it are slipping through our fingers rather rapidly. It's much harder to do now than it would have been a year ago, and five years from now I think it would be essentially impossible. But the people are still available, the experience is still fresh in their minds. There would have to be a good deal of direct questioning on the confidential level

Weaver: to get the kind of answers that one wants. Something awfully important could be gotten out of the study of operational research in England and the United States. I'm not suggesting that such a historical case study should replace the research work. But I would have more interest in the examination of the actual experience of what actually happened under various sets of circumstances than in a purely conceptual analysis of what might happen. It would have much more of an air of reality about it, particularly because there actually is such a wide range of experimental situations available for analysis, and with a record which was thoroughly tested by practical experience.

Kris: The problem should be divided in two - one being particularly amenable to historical research and the other being more amenable to experimental methods. Surely there are cases where individuals would not profit from being in a team. I would be very inclined to think it profitable to at least negatively establish personalities which do not fit into teams regularly. The selection problem of people fit for team work has been such a great success during the war, that I think it is only obvious that we learn from that experience and construct a revised test for scientific team work. The Beon (?) test was created by the team work in Harvard and then applied by Beon (?) in Britain; we might also study the OSS selection test. An experimental procedure would amongst other things clarify what people do not fit, and on the contrary, are likely to disrupt teams. Naturally an experimental situation does not pattern after a war situation, where people are on the whole likely to behave a little better.

Helmer: On the one hand, there is a problem concerning personality structure, the problem of finding out what people are suitable for teamwork and what people are not. On the other hand, once you have a number of people whom you know work well in teams, there still remains the question of how many of them should work together on a given problem and what the structure of the team should be -- if one person ought to be in charge, if it should be entirely democratic, or if there should be several levels within the team.

Stouffer: This illustrates just exactly the kind of course which we're going to have to pursue to if we're really going to develop an experimental social psychology. It would be very sterile just to sit down in the armchair, develop some hypotheses and test them in the laboratory, if there is a large body of experience that can be drawn from. I would be very much in favor of setting up a project and spending a good deal of money on it to tap this accumulated experience in wartime of the operation of these teams. I wouldn't stop with that. There's going to be so darn many uncertainties in that situation, that out of it it might be possible to formulate rather sharply some kind of hypotheses which we can put to experimental test. I've got a lot of faith that we're going to make an experimental science out of social psychology. In the study of small groups we have the kind of setting in which it is possible to conduct clean experiments neatly and rather economically, with enough replication so that we'd have some confidence in the results. Whether this particular project is approved or not, the Harvard Laboratory is going to be doing a lot of work of this type in the next few years. That's one of the things we're interested in doing. I don't know whether we ought to take on this kind of project. I'm not sure that we're ready for it yet, or whether we should work at a somewhat simpler task than might be implied by this -- this implies teamwork in scientific projects, but it might be better to work on teamwork in crossword puzzles or something like

Stouffer: that to start with. I'm very much inclined to think it would be very uneconomical to attempt a project like the one discussed here without tapping the experience that has been accumulated; there ought to be some way to get that experience assembled. I don't think you can prove anything much out of that experience, but I think you can get new ideas and new hunches. I don't think you can prove too much out of experiment alone, either, but something out of a series of experiments. The real test will come when these experimental findings are actually applied in new types of situations. This is an example of how you can work -- a historical approach and an experimental approach side by side, so that each of them greatly illuminates the other. I, for one, would strongly favor careful consideration for this kind of thing.

Helmer: I like very much what you said and feel very much like you. One ought to use every opportunity one can within the social sciences to conduct designed experiments and at the same time use the information that has been accumulated in the past.

Mosteller: I'd like to emphasize what Warren Weaver said. If it is thought that this historical project is within the scope of RAND, you must decide almost at once if you're going to do it, and get at it. If it has any chance at all of achieving anything it's got to get at its work right away. Nobody is going to remember how it was, and past labors grow easier and easier as time goes on. If you have to gather this information by the interview technique, you're going to find that everybody has fond memories of something that they disliked very much at the time, and nobody remembers exactly the terrible things that happened, nobody remembers how the material they worked on wasn't used, nobody remembers how the liaison wasn't there when it should have been, etc. Whatever good data there originally was about the troubles of working in the particular heterogeneous groups that are involved in operational research will be dead. And as Weaver said earlier, it's almost dead now, so you've got to get at it soon; otherwise it's hopeless.

Stouffer: This is like the anthropology of the vanishing race.

Barnard: I'm not at all sure it's not like the genetics of the carrier pigeon. If you're talking about group operations in the war, that's true; but I don't think you need to assume that such operations aren't going on all the time in great profusion. Hardly anything of any magnitude is done today that doesn't involve the kind of thing we're talking about here. But you have to distinguish between problems which call for decision and problems for knowledge. You've got scientific research and another type of research confused all the way through. Another aspect of the matter is this. The present formulation puts the emphasis on composition, but as much emphasis should be put on the technique of group operation. The most unusual characteristic of the operation was that there never was argument in months of the most excruciating work. I had a lot of experience with that kind of thing in U.S.O., where you have people of different faiths, races and classes trying to get solutions of problems that had to be stated in abstract terms leading very easily to a lot of dogmatic discussion; and that also worked out. I would think it desirable to approach this thing from both standpoints - the technique of operation as well as the composition of teams, and both on problems of reaching a decision (which certainly is important from the Air Force standpoint) and problems of getting scientific knowledge.

Mosteller: In our first pilot study, we deliberately designed into the problem two parts. For the first part, a decision had to be reached; the second part was a solution of the problem on the basis of that decision. That has something to do with the two problems that Barnard was talking about; it can easily be done with such an experiment.

Barnard: Considering the scope of projects presented to this Conference, this project is very well worth putting emphasis on. On the collecting of data concerning past experience, the practical method is to get the observer, if you can, into the situation without his being a disturbing influence. The only other method I know of is to get a report from people who are in it, and those reports will be extremely unreliable.

Stouffer: You could get a great deal from this non-participant observation. They've also been trying to work on some techniques to get relatively objective categories of classification, so the observer can summarize what he sees without resorting to a lot of anecdotes. After observing the operations analysis teams in the Air Force (which I had a fair opportunity to do during the war), I'm sure I would not rely on their reports on what happened, on their relations with their associates, headquarters, or home office, or on the problems that they had. This is very difficult.

Weaver: I'm sure that it's difficult, but I'm not convinced that something can't be distilled out of it even yet. I think there is a technique of observation and analysis that would get somewhere on it.

Barnard: Before you get through with this character study, you get at one of the most important unsolved problems -- how you reach a correct solution to a considerable proportion of problems where you are dealing with situations in which not all the variables are known and most of the variables are not measured, which is the actual situation of military or business institutions. After trying for fifteen years, I've never been able to get my own mind straightened out on the limits of what I call the logical use of the mind and information. I'm still waiting to see someone deal with that question. It's at the root of most of this problem, I think.

Stouffer: We want 20 years before we're going to promise very much on this. I'm serious about this. But by very intensive work I think in 20 years we're going to know something. I don't know what anybody can do in 2 or 3 years that will be of much help. Whether RAND should go into this at all is a hard question to answer. I don't think we should kid ourselves about the magnitude of the task that we're undertaking.

Henderson: The next project, on training for cooperative research, confuses again the questions of advancing knowledge and of trying to reach decisions as to practical applications.

Weaver: I am so foolish as to think that people in all intellectual fields ought to work in the way which they find congenial. The wide range of results that one gets from letting them work in the way which they find congenial is all to the good. Nevertheless, it seems to me fairly clear that there are problems facing us now, probably largely in the field of biological and social sciences, where the type of complexity and the number of non-quantitative factors is such that a group attack

Weaver: on them is the only one which will produce results. I don't think you can by frontal methods train people to do that. I'm completely skeptical about frontal attack, so to speak. I think the way this is going to happen is by infiltration. Groups are going to work as groups in universities much more, and people are going to find that that is a convenient, natural and happy way of working. A set up for a project of frontal attack on training people to work in groups in almost as futile as setting up an organization to teach people how to be happy. All you do is bore them.

Helmer: In many cases it isn't a question of forcing people to work in teams but rather of giving them a chance to do so. When I first began to do some work within groups in the Applied Mathematics Panel, it came to me almost as a relevation how much can be achieved by a group. I was never quite aware of that. We are much too much brought up in the spirit of working in an ivory tower in academic institutions.

Weaver: You learned that, not by taking a course on the advantages and disadvantages of working in groups, but by working in a group. The way in which to teach people that it is pleasant and effective to work in groups is to give them an opportunity to work in groups, not to make a frontal attack on the problem of training *per se*.

Stouffer: I may agree with Weaver as to this particular project, but I'm not sure that I would agree with a rather dogmatic view that you can't train people to work in groups. Mosteller and I ran a seminar last year in which we made the participants do group projects with a division of labor; and we made those people work in committees. We even graded them in the course, in part, in terms of the cooperativeness with which they worked in groups. Some of them had this same revelation that our chairman is speaking about, and they had it in their first or second year of graduate work. That is the way to the future of social research. We darn well ought to begin way back in the very beginning of graduate work, giving them the experience of the give and take of work with each other. I don't like to be too dogmatic here on the view that you can't train them early.

Mosteller: I do not think that this is a RAND project. On the other hand I agree fairly heartily with what Stouffer says. The way people find it congenial to work is the best way, and the way they find it congenial to work depends quite a bit on how they found it congenial to work in the past. If you give them opportunities early enough, they'll learn to work with other people. I think also that the infiltration idea proposed earlier is a good one, and there are many ways to infiltrate. Hendrick Bota (?), John Tukey, Charles Windsor and I have been writing a paper that we call (provisionally, since this is only the second draft) "The Education of A Journalist". The general idea is this. One difficulty that people are beginning to notice is that it's pretty hard for some kinds of people to talk to other kinds of people. For example, it's rather difficult for social scientists to talk to nuclear physicists. As a first move in trying to get people who can talk to one another, it seemed to us that you might deliberately go about training a journalist - a man who wants a liberal, scientific education and a modern one. We've been drafting a paper on how this can be done. It can be done very easily under the present set-up in colleges and universities with just one change -- that the man does not have to enter a specific department. There are something like four "axioms" involved: (1) that any course may be replaced by a more advanced course in the same department, (2) that skipping of the usual prerequisites to a course is not only encouraged but

Mosteller: expected, (3) that minimum academic standing must be at least one full grade above that required in the usual curricula in that institution, and (4) that inter-departmental transfer should be allowed. (As I say, I don't think this is a RAND project that we're talking about here.) There would be a good market for this kind of man, if he's good and has training in quite a number of different fields. I move that the project itself be abandoned as far as RAND is concerned.

Helmer: Project 11:2 on Reliability of Prediction is of very profound concern to Project RAND. As you know, we are charged with the responsibility of making recommendations to the Air Forces on a pretty high planning level, and in making plans which ought, at any rate, to concern the relatively distant future. It is necessary to make recommendations on the basis of expert guesses -- that's really all they can be called. The question is whether there are any systematic methods by which one can get the most out of various guesses or the various experts available. The importance of the problem is beyond doubt. There is the question of the selection of men and also of the optimum combination of the estimations made by different people. This second part is presumably a problem in theoretical statistics.

Kaplan: It would be useful to appraise reliability of predictions in conjunction with indications of the bases on which the predictions have been made. It would be useful to have the predictions associated with judgments by the predictors of the confidence with which they are making them, so as to obtain a second level appraisal of the estimate. I venture to say that one could in a short time arrive at a number of possible specifications, in addition to combining results or selecting personnel, which, even if they did not increase the reliability of the forecasts, would at any rate tell us what confidence could be placed in them. That in itself would be no small gain.

Marquis: Predictions are always made on the basis of some theory or principle applied to a particular situation. The usefulness of predictions is related to the development of theory. I can see now, particularly in the context of this project, that very often we make predictions in the absence of any objectively stated theory or principle. This is the "intuitive" type of prediction, the kind that a physician makes when the diagnostic rules fail him and he proceeds on a hunch. There may be ways of improving the reliability of such predictions other than by refinement of the objective principles and facts on which they're based -- such as by combining several predictions each of which is "intuitive". I take it that this project is concerned with that, rather than with the development of objective principles which would improve the predictions.

Kaplan: It is quite true that from a logical standpoint successful predictions must depend on some theory about the subject matter. Improvements of theory involve by definition a heightened reliability of prediction. There is another problem however, which relates not so much to prediction as to what's commonly called "judgment". Possibly this distinction that I'm about to make is very like the one which Barnard introduced earlier between a scientific problem and one of arriving at a decision. There are many instances in which the expert must be consulted, not with regard to the occurrence or non-occurrence of a state of affairs, which can afterwards be verified and for which some frequency of successful prediction obtained, but rather with making a decision in a state of affairs which will never recur, and one moreover in which it is very difficult to appraise subsequently

Kaplan: whether or not the decision was made with good judgment. I would suggest that, when this project arrives at the stage of the design of research, thought be given to the possibility of dealing also with this other area of making judgments. It presents different sorts of problems. We don't appraise an expert's guess solely in terms of whether it's right or wrong. There are such things as valuable errors. It may be that the prediction is mistaken in a very reasonable and useful way. I should suppose that in the practical type of problem to which Barnard referred, this faculty of judgment might be equal in importance to the ability to predict.

Kris: The predictor very frequently has an implicit theory if not an explicit one. The so-called "intuitive" forecast is based on hypotheses applied without being made explicit. It would be very interesting to find how people who have made successful forecasts arrived at them. That I would consider something very valuable, from which we could all learn. Certain highly endowed and gifted novelists have anticipated changes by twenty years. I was wondering whether anyone has ever investigated such forecasts, even only in the field of technological progress. It is astonishing that so much has been correctly forecast; it would be nice to see what has been erroneously forecast. One could possibly get some sort of insight into the whole area of what I call "fantastic forecast."

Henderson: I agree with the first part of what Kris said. It's very important to realize the full complexity of trying to analyze the theory behind a decision. In the case of hunches the reasoning may be more rational and based on greater experience and better knowledge of the field than is often true where a carefully stated explicit theory is given as the reason.

Helmer: The reasoning behind a person's forecast is absolutely essential in such an analysis. Take, for example, the forecast as to the year when Russia can first be expected to have atomic bombs at her disposal. It's a very important question which effects the national policy, and particularly the policy of the Air Forces, a good deal. If you simply canvass a number of people whom you consider experts, you will get a variety of years -- a number will say the Russians already have it, some will say 1955, some will say 1970, etc. If you take something like the mean or the mode, you will get nowhere near the truth. You won't get anything that will be of great help. But if you analyze the reasons that these people may give, you might get somewhere.

Kaplan: I take it that the significance of the remarks which Kris was making was that one cannot attach too great weight to the reasons which are actually given by the person making the judgment. In the cases which we call "hunches" or "intuitions", the individual is unable to specify those reasons although in fact they may be good ones.

Barnard: I am thinking of two men, both of whom over a long period of years were rated to have extraordinarily good judgment, usually not expressed in terms of predictions but in terms of action which implicitly involves estimates of the future. One of them is almost unable to express himself about any subject; I have never in all my life met anyone, except a dumb person, so incapable of stating any reasons for anything. The other is the greatest rationalizer I know of. I tell him every once in a while that if his judgment were no better than his rationalizations, it would be impossible. His judgment is very good, but his reasons are always bad.

Henderson: Helmer was referring to a problem which is in some degree susceptible to analysis. Your prediction of when the Russians can have the atomic bomb depends on your estimate of the ability of the physicists, the amount of emphasis the government is putting on it, the industrial capacity, ability to do certain kinds of things, etc. The reasons given by a group of experts who are asked to give a date may be very important, because they may be able to furnish a basis for further examination of the situation in Russia which will enable you at some later time to make a better prediction. But whether these reasons have anything to do with the validity of the date they give is very much an open question.

Mosteller: I suspect that last little remark isn't true by a good margin, but I agree with some of the earlier points. Could we summarize it all by saying that we think that some specific aspects of this problem are really appropriate for RAND, that RAND has facilities for beginning at least a pilot study on those special problems, and that there is no reason in the world why they should not go right ahead with it?

Weaver: I would register a very mild and humble disagreement. It appeals to me as a very interesting topic, but I would guess the probability of yield of studies in this area to be relatively low, as compared to one or two things we spoke of just previously. I'd be very happy if some bright people were working on this under their own steam, but if anybody asked me whether I thought it was worth spending X thousand dollars of somebody's money for this, I think I would hesitate quite a little bit. I think it is interesting; it may come to something. But I think the chances are relatively slight.

Helmer: I'm surprised at the position that Weaver is taking because it seems to be in flat contradiction to some of the things he has said in his pamphlet on Aerial Warfare.

Weaver: No; I think it is interesting and may turn up something useful, but I would guess that the probability of yield is low.

Kaplan: Are you speaking now of both the projects that have been differentiated earlier -- the study of forecast behavior as well as the more specific one in which some attempt is made to increase the reliability of prediction?

Weaver: I speak of the second as well.

Kaplan: I'd like to register some feeling to the contrary (only because the topic of making judgments without being able to give good reasons for them has been so much before us!) I can't say that I have any basis for a high expectation, but the matter is so very important that even with a low expectation it would be worth seeing what we can get from it. Suppose the results were limited even to the statistical question of whether or in what kinds of circumstance you want to use the mean or some other measure for certain types of guesses. Even that result would be of extreme value, and I would not expect that one would have to go through 20 years of research to get some light thrown on that sort of question.

Weaver: I can produce an instance in which I'd be quite confident that the mean was the best -- if you were having 20 people guess the length of a table that they

Weaver: hadn't ever seen.

Kaplan: There is some room for doubt on that, but with little difficulty we could specify other questions of comparable simplicity on which there would be room for doubt, and about which it ought to be possible at least to gather some preliminary data.

Stouffer: I can cite an example of that type of situation. A study was made of estimates of scores of football games. It was found that there was always a tendency for people to be too conservative about the scores, because nobody wanted to stick his neck out and say seven touchdowns. The attempt was made to generalize that result in a paper in the *American Journal of Sociology* about 10 years ago, that the mean of predictions tends in social situations to be too conservative. I don't know whether the psychology was right or not; I'm mentioning it only to illustrate this problem. I think I'd go along on this. I'm not too sure that I can see it as clean-cut enough to justify setting it up as a project. I'd like to see some bright person fiddle with it.

Henderson: A mean of the guesses of a number of people is a rather ridiculous way of handling the problem, because you are bound not to be able to find people who have the same experience or intelligence or knowledge of the situation. This leads me to go along with Weaver in thinking this is something it would be nice if somebody else would do.

Helmer: I'm sure that the question of whether a mean should be chosen would have to be investigated.

Henderson: There is also the question of rating experts.

Helmer: I would like to come back to Weaver's statement that the yield of such an undertaking might be extremely low. I'm inclined to agree with that, and yet in spite of that I think it would be extremely important to carry this out. Assume that our policy makers of the moment make the right decisions in, say, 60% of the cases. If on the basis of such a study one could help them to make the right decisions in 62% of all cases, don't you think that a few thousand dollars would be well spent?

Weaver: I do. If I thought there were any reasonable chance of arriving through such a study at a theory which would, when advanced, be sufficiently attractive to persuade the policy people to use it and have the practical effect of raising the level of their predictions by 2%, I would vote for it. I just consider the chance of those two things happening in conjunction to be awfully low. As compared with some other projects here it isn't one that I put very much confidence in.

Helmer: I don't think it's so much a question of persuading the policy makers as of convincing them that the result of such an undertaking might guide RAND in its recommendations.

Kaplan: It may be that there is an inter-mediate position. It might be possible in the course of RAND's future operations (and possibly one could interest other organizations which have frequent recourse to expert guesses) to request, in conjunction with the regular solicitation of such guesses, one or two other specifications which might build up useful data. For instance, one could with very little

Kaplan: difficulty ask the expert to give a specification of the degree of confidence which is attached to his prediction. The accumulation of these materials in themselves might at some future time afford a possibility or at least a suggestion for more seriously designed research.

Barnard: I agree with Weaver; I'm against it. There are more complications that would need to be taken account of. One is that a man's judgment depends upon whether or not he is carrying responsibility. Some men are very good in their judgments if they are not carrying responsibility, and they are perfectly cockeyed when they are. They are like the fellow who's expert at golf but can't play in a tournament.

Helmer: Project 11:3 is A Survey of the Need for a Reading Machine. The question of the usefulness of such a machine was briefly touched upon in a different panel in connection with content analysis. As you know, in order to conduct content analyses it is necessary to go through a tremendous volume of written or spoken material; the manpower required to do that sort of job thoroughly is simply fantastic. It would be valuable if it were possible to do it by an electronic machine which could automatically read a book, so to speak, and record what it reads. As it scans the lines of the page it might transform each word it reads into a punched card. They could then be used for various purposes. For instance, if you are interested in the frequency of occurrence of certain words you simply use an IBM sorter. Williams suggested that you might rearrange the words alphabetically, then, if the material is in a foreign language, translate each word by use of the standard dictionary, and then return to the original order; thereby you would have obtained a very rough translation. Of course it would be *very* rough, but still it might be sufficient to determine roughly what the content of the book is.

Mosteller: The need for this machine is very clearly indicated already. Some people are already working on a design. It is quite clear that this machine is necessary. You don't need to survey the need. I don't see why a social science organization should stop to consider the necessity for this machine. This is going to be a useful gadget when people get it, and better ways for using it than anybody here will describe will appear as soon as we get it.

Helmer: There is some little gadget, I believe, in the form of a stylus that you have to guide across a page which can recognize letters.

Marquis: It changes them into auditory signals. It was developed primarily in Bell Labs and RCA for reading by the blind. To put that auditory signal into a punched card would be the next step.

Weaver: The fact that you have to guide the thing along the line isn't really serious; there isn't an awful lot of engineering between guiding the thing along a line and having it move automatically along a line.

Stouffer: The one thing, I understand, which has not been accomplished is to translate the electrical signal from the photoelectric scanner into a device to make a "talking book". They can't turn it into a verbal auditory signal.

Mosteller: They are working on that.

Stouffer: I want to dissent strongly from one statement that was made -- that it isn't the function of a social science group to deal with gadgets which engineers make. I disagree with that violently because the progress of social science really depends on two things. It depends on the development of operational concepts in social science which are testable and tested, and it depends on the development of gadgets like the IBM machine. It's up to social scientists to stimulate the development of machines, gadgets, and tools that will speed up the work we have to do. If somebody comes up with a project that involves spending some money for making machines, that might be the most economical way possible to step up social science research; and I think as a social scientist I'd plug for it.

Mosteller: If I get a lot of data -- I mean a *lot* of data -- and I want to use that data on an electronic computing machine, for example, the only way I'm ever going to get it fed into the machine accurately and fast enough so that it does me any good to have bought this electronic computer, is to have some other gadget that will put the material in the machine. There is no use having a machine that does something in a micro-second if it takes a year to get data ready for the machine.

Kaplan: Among the great objections to content analysis is the enormous scope of the operations that have to be carried on even for the simplest kinds of statistically reliable results. Such a machine, as is the case with all scientific instruments, is of value not only for the uses which we have for it in the present state of our investigations, but because of the many lines of research which had not previously been considered because they were impossible on the face of it, and which then can be seriously entertained.

Helmer: Project 11:4 is a more general one on the Application of Electronic Machines to Social Science Research. Would it be worthwhile to canvass a number of social science workers for suggestions or ideas for electronic machines not yet designed or for the employment of electronic machines already in existence?

Weaver: The computers that exist are so jammed up with work that you can't get into them. And there are a very large number of people who are designing new machines.

Helmer: Project 11:5 (Elicitation of Research Ideas) might be considered somewhat imaginative in the derogatory sense of the word. However, I think it should not be dismissed without at least giving it a little thought. Some social science projects have been considered in this Conference which fall in this imaginative category, for instance -- and I think this is a very important and serious example -- the project concerning the selection of individuals as targets. This is the sort of thing that has not been given enough thought in the past. If what matters in war is to get the enemy to surrender, and if this can possibly be done by eliminating perhaps a dozen or a hundred individuals of the enemy elite, this might be by far the cheapest way of winning the war, both for the victor and the vanquished. This seems to me an imaginative idea on the same level as some of those produced by natural scientists in the recent past. I can't help getting the feeling that there is a good deal of defeatism among social scientists. Somehow that ought to be overcome. A great many social scientists don't let themselves go imaginatively.

Weaver: What do you mean they don't let themselves go? Do you think that Buck Rogers advanced physics, that Jules Verne and H.G. Wells advanced science because

Weaver: they made all those beautiful and successful predictions? I don't see it. I think there is something being confused here. Social scientists could go H.G. Wells and Jules Verne if they wanted to, but what good would it do? Although one could produce an example that's on the other side. I'd say that Goddard did really advance the whole rocket art. For years he was working pretty nearly on a crank basis as far as other scientists were concerned, and yet I think you'll have to agree that Goddard really advanced the art. So that there are cases in which rather stubbornly imaginative individuals have contributed to science and technology.

Marquis: There is a very good mechanism for eliciting imaginative ideas in physical science, namely, invention through the patent system. And there are journals like *Popular Mechanics* and others which stimulate a flow of ideas from which there are presumably some good results. I should like to know what the experiences of the scientific councils during the last war were. I remember they put out lists of things needed; I don't know whether they got anything back or not. I'm told that they did get a few good ideas, but I'm not sure. But there is no such mechanism in social science for social inventions or even research ideas. There is no patent system possible for them, so far as I know.

Barnard: If you're talking about research in the sense of getting knowledge, what you say is true. If you're talking about research into methods of improving the control of behavior, there is an awful lot of it going on right now. My organization is full of it -- staff men whose only job is to dig up new ways of doing things with people.

Mosteller: I have one idea, and that is not to limit your horizon. For example, if you are going to solicit opinions, I don't think the people you solicit them from are necessarily the social scientists. You ought to talk to physical scientists or the laymen, or somebody else. I've had a little experience as a magician, and a couple of times I wanted to invent a new trick. And if you talk to magicians about inventing a new trick, they tell you the same old "take a card" that you had before. When you're looking for these bright ideas, especially in a field like this, maybe you don't want to talk to people who know all about it. Maybe you want to talk to somebody else.

Helmer: Isn't this mainly a question of cross-fertilization of ideas among natural scientists on the one hand and the social scientists on the other?

Mosteller: Why should I restrict them by cross-fertilizing them? Why should I drag down their imagination by explaining things to them so they'll know what I can or can't do? Why not let them tell me what they'd like to have me do, then I can worry about whether I really can do it. I'm talking now as a social scientist rather than a magician.

Marquis: There might be an announcement in a popular magazine like *Colliers*, with prizes; and then just put people to sorting the responses. I agree with Mosteller completely that you want to go well beyond scientists to look for the original lines. I think, for example, of the person who wanted a substitute for a baby-sitter, and went to a physicist to ask for it. It took a lay person to think of it, and he got one -- a magnetic radio.

Henderson: If you ask for a device which will accomplish a specific purpose -- a new magical effect, a substitute for a baby-sitter, or something of that kind -- you have a possibility. But you cannot simply say you want imaginative ideas in social sciences in an announcement in *Colliers*. I don't quite see for myself that you are seriously considering some such procedures. What form would the question take? Certainly it would be hopeless to ask, "What ideas have you to help prevent or to help win a war?"

Weaver: You have to say something and you have to say something somewhere. Now, what do you say where?

Helmer: Person to person stimulation and encouragement may be important. The social scientists should not shrink away from an idea just because they feel that there might be some physical gadgets involved which are impossible to construct. To mention two examples which Rosten brought to my attention: it was suggested that sound or darkness might be used for purposes of demoralization in warfare. The social scientist might say, "Why should we even bother to think about this -- there won't be any physical methods of carrying it out." In the case of sound it probably can be done, and in the case of darkness it can perhaps be done. This is the sort of thing that seems to me to show that imaginative ideas are often squelched because people don't quite realize the potentialities in some other field. The same might possibly hold for the application of electronic machines in the social science field. I have a feeling that many social scientists are not aware of what can be done by means of such machines. And they won't allow themselves to think along lines which are entirely within the range of the possible today.

Weaver: Most scientific discussions seem to be in terms of the logic of the final tested results -- not in terms of the processes gone through to get the results. There are probably ways to stimulate imagination and darn little is known about it -- and ways to test people who have imagination by a Rorschach test of some kind. But it's premature to appraise them.

Helmer: The last project is an attempt to set down a concrete proposal for stimulating ideas by means of a newspaper search for research suggestions. This is a standard technique applied, for instance, by radio companies in order to get ideas for their programs. They have a number of readers who scan the newspapers regularly and collect ideas that could be used, for instance, for crime programs. The question is whether this method might possibly be used to produce proposals for research projects within the social sciences.

Kaplan: Without prejudice to the specific suggestion, it should be pointed out that the people to do it would have to be extremely good. It is a matter of having the kind of imagination which would be stimulated by these materials. Possibly the greatest difficulty would be the one referred to in the previous instance -- finding people who are really imaginative. And if they were, perhaps they could do without the newspapers.

Weaver: It would be interesting to have some social science equivalent of the *Amazing Story* pulp magazine. It is a fact that a rather surprising amount of the technological invention and, to some extent, of the scientific developments applied during the war were pretty well catalogued in the pulp magazines over the five or ten

Weaver: year period preceding the war. If one had carefully read the pulp magazines for the ten years preceding the war, he would have had a fairly good catalogue of the gadgetsy of this war.

Kaplan: Suppose it were suggested to the editors of the various scientific fiction magazines that they invite fantastic stories in which the fantastic element centers around, not so much the physical or the biological sciences, but the psychological and social sciences? Very possibly they would reap a comparable harvest.

Helmer: With respect to Weaver's remark -- this has been going on since the war, too, of course. Someone in RAND had a rather ingenious idea recently which appeared to be quite promising. He was very proud of it until one day we bought a copy of *Amazing Stories,* or *Astounding Stories*. There was an exact description in detail of his proposal which, I might say, is still an active project, and, of course, very highly restricted.

Mosteller: I read the scientific fiction magazines occasionally, and I find an average of one or two good social science fantastic matters in these stories each time. If you had one person on a project who was interested in that, you could let him spend a couple hours a week reading over those things for us, and see what happens.

Stouffer: I've been trying to think about what social science needs most in order to speed up its progress, and I'm convinced that one of the needs is better social psychological measurements. There's a good deal of work going on in this particular field. During the war some new techniques of scaling were developed which represent a brand new approach to the whole problem of social psychological measurement. These, in turn, have stimulated both criticisms and other new types of developments. There is an approach now by way of factor analysis that looks very promising. I think we're just at the beginning here of a period when we're going to see a flowering of work in this field. A lot of rapid progress should be made by examining the logical bases, mathematically, of various models that might be constructed for measurement in social psychology, applying them to a large variety of data, working out the situations in which one model is different from another, and trying to find out what can be done about it. For example, the model we worked out in the War Department will yield a scale only under conditions under which the factor analysis model to which I referred will not yield a scale, and *vice versa*. That just can't be if they're both measuring the same magnitude, and yet, both of them have excellent logical and mathematical bases. Mosteller and others are working now on the problem of trying to reconcile them, and get them into the same system of some sort. We're just at a time now when the kinds of stimulation which Project RAND might very well give might really speed up by several years the process of development here. This is utterly practical, as well as being basic for the development of social science as such. I would like to suggest the desirability of this as an explicit project which might be farmed out to several universities -- to stimulate the work in those universities, and to develop better and more rapidly their methods of social psychological measurement. I think we've got something hot, and I think we ought to follow it and follow it quickly.

Marquis: Stouffer's statement is quite accurate and factual. I concur also with his belief that we should stimulate the hot areas which are reasonably certain of rapid development, as well as looking for fantastic new paths to follow.

PANEL XII

ORGANIZATION AND UTILIZATION OF RESEARCH

W. Weaver (Chairman)

C. Dollard
H. Goldhamer
O. Helmer
A. Kaplan
D. Marquis
F. Mosteller
S. Stouffer
S.S. Wilks
D. Young

Young: Concerning Project 12:1 on Integration of Research Activities, I'm not much interested in the widespread solicitation of opinions in such matters. The problem has not been entirely neglected. We have various efforts under way at the present time, ranging from mechanical devices like abstracts and yearbooks through the Social Science Research Council. I'd like to see the problem broken down into much smaller questions which could be worked on. For example, there is no adequate abstracting service in the social sciences. There was one tried, the old Social Science Abstracts, which ran for four years and failed in terms of aiding research. The history of that has been written up and is available. The United Nations is now preparing more evidence on the need for international abstracts in the social sciences. There is the problem of the organization of the social sciences, which is very poor. Social science societies do nothing but publish a rather nondescript journal and have meetings once a year which gratify the ego, and then they have some fake committees. We need better organization of some kind or other. Perhaps the best organized of the social sciences is statistics, and that has many defects.

Wilks: The main problem here is the following: Are all the scientific laboratories in the country who are concerned with national and international security problems happy with the arrangement they now have for getting information through the Office of Naval Intelligence, Army Intelligence, and the State Department? The real problem is whether the information flowing through those three agencies is satisfactory from the point of view of all of the research laboratories in the country. If the situation is not satisfactory, one certainly ought to be able to find that out by talking, say, to the librarians of these various laboratories. And if it's not, then the problem should be one of needling these three agencies.

Dollard: Perhaps one of the most useful things to do is make an analysis of the experiences in trying to integrate research activities. Young has spoken of all sorts of attempts at abstracts, journals, etc.; it would be salutary to know to what degree each one has succeeded and why those that have been dropped were dropped.

Young: Another problem is this. Social science is non-repetitive and non-cumulative -- once a study has been done on a particular problem nobody ever repeats it, as he would do automatically in biology, for example, and furthermore, they don't build on what has been previously learned. Studies of the same problem are usually so

Young: designed that you can't check one against the other. Some years ago the SSRC asked Dorothy Thomas to compare the leading studies of migration differentials. Out of 212 of the best studies she could find, I think 6 were found to be in agreement if you reworked the figures. Each writer had shifted his definitions a little bit, so that the studies didn't cumulate. One way we have been trying to correct that is by research memoranda covering what is known on some specific problem -- the kind of job that Sam Stouffer did for us on predictive techniques or Robert Sears on psychoanalytic concepts. That has been reasonably successful. It has given to people working on those specific topics a concept of trying to add, and a sense of the problems which are next in line. We don't think about that very often in the social sciences -- what is the next problem to be faced from where we now are? That kind of job could be done, or at least that problem could be studied further.

Dollard: What Young said was that any evaluation or listing of what is going on in any field must be qualitative rather than quantitative, and I have not yet seen any system of recording research which has that virtue.

Weaver: When you speak about the possible usefulness of bibliographic or abstracting techniques, indeed the whole mechanism of improving the accessibility of what has already been written down -- that is a perfectly definable problem. It is a graveyard, as Dollard points out, of dollars and spent lives in efforts that have not succeeded. There are a few very significant instances in which it has succeeded very admirably. There are instances in which the patient was just about to die and was then resurrected and has gone on to live a very happy and useful life. I would cite Biological Abstracts as perhaps one of the most significant instances.

Helmer: From my own experience the method of abstracts is useful, but it is not quite sufficient. In my work at RAND I get dozens of bulletins and abstracts across my desk every day. It's much too many. I don't have the time to read them, and they are usually written by people who don't really know the subject well enough to make a good abstract. There is no guarantee that the abstracts in any way represent a complete bibliography of the real material. What is needed in addition to the system of abstracts is something like a scientific intelligence service -- an organization to dig out information as to what has been done in a specified field. The only way for them to find out is to communicate with a number of organizations that might have done some work in that field and gather together all that is known on the subject.

Weaver: This whole broad question of how man can get himself out from under the weight of the printed word and effectively get into contact with the microscopic portions of it that he'd like to come into contact with is an enormous one. It is one which certainly is capable of extremely rapid and clever mechanization at the present time. The thing that kills these gains is the problem of classification. The problem of the classification of the sciences is one at which philosophers and scientists have worked for hundreds of years without success. When you take into account that you have to set up a classification scheme which is infinitely flexible, capable of infinite readjustment as new materials come in, the problem becomes hopeless.

Goldhamer: The question raised about classification doesn't really meet the suggestion that Helmer made. I interpreted it as having its chief merit in the fact that it really evades the question of classification. We do not attempt to set up an all-powerful, all-inclusive bibliography -- something like a master Social Science Abstract -- but

Goldhamer: rather search only for things for which we have an immediate need and whose character we can within some limits specify. Some sort of what Helmer called an intelligence service could be extremely valuable. Frequently you know that certain materials exist, but not where to put your hands on them, and you waste a tremendous amount of time digging around. Even if this intelligence service did very little more than provide, say, a certain amount of stenographic service, and have friendly relations with persons who might be considered good leads or even have systematic reproducing services -- those things certainly would pay off, trivial as they might seem.

Mosteller: There has been some talk about communication activities being costly in manpower and money. I certainly think it is costly in money; I do not think it is necessarily costly in research manpower, however. People who are not doing effective research can still do effective work on abstracts, reviews, and communications.

Wilks: As an effective tool for the RAND organization, this is simply a RAND library problem. The Navy Operations Analysis Group had probably the finest library in the world on its topic. They had a librarian there full-time who did absolutely nothing but think in terms of every possible connection the Operations Analysis Group could establish with the British, the Army, the Air Forces, the State Department. This librarian saw that his organization got on everybody's mailing list. They had a fine library, and could keep their fingers on everything that was going on. If we are talking about the general theory of scientific cooperation, that is much more general. There the question is -- are all the scientific organizations cooperating with the Army, the Navy and the State Department getting what they want? If not, then maybe RAND could needle the agencies so that they do. I'd separate the problem into those two parts.

Weaver: If one of the problems of the RAND organization (and I should think it is an important problem) is to set up the most effective mode of communication internally and externally, they can solve that problem by consulting two people. The librarian of OAG would be one of the two; the other would be John E. Burchard, librarian for Division II in Terminal Ballistics. This probably had the finest technical library in the country, and did the finest job of establishing connection with every organization in this country and elsewhere for their technical problems. John Burchard is himself a scientist as well as a librarian. He made the Directory of Libraries at MIT. I think he understands the problem of organization of technical service of communication for a highly ramified set of problems of this sort about as well as anybody in the country. The broad problem of communication and integration of results and information within the field of research in some broad intellectual area is a very different problem. I judge that we all agree that there is a limited and concrete problem that applies to RAND. This is a problem of internal organization of RAND, rather special in character, which they can solve in terms of experience that they can easily tap. In a very broad sense, there is the problem of effective organization of research in any field. I assume that everybody here quite generously agrees that this is a topic which requires improvement. Whether it can be improved by a frontal attack, I'm not at all clear.

Project 12:1b is on Scientific Personnel Resources. It is a study of what can be done to alleviate the present scarcity of competent scientific personnel. How much manpower can be saved by proper coordination of research, what type of Federal subsidies should be recommended to alleviate the situation? This poses an obviously

Weaver: important problem, on which a fair amount of intelligence has been directed. The personnel committee of the group that prepared the Bush Report under the chairmanship of Henry Moe and under a large amount of inspiration of James Conant did, I would say, not a wholly amateur job. The Office of Scientific Personnel of the National Research Council under Trytten is doing a good job. A study is being carried out under the joint auspices of the Social Science Research Council, the National Research Council, the American Council of Learned Societies, and the American Council of Education. The Steelman Committee's report should be mentioned in this same list. That's a fairly big mass moving with a fairly respectable velocity. Does that represent a momentum with which you gentlemen are content?

Young: We did not do too well in World War II on our national roster of scientific and specialized personnel. At the present time you have Trytten's Office of Scientific Personnel preparing lists of highly competent specialists in the natural sciences. Social science is going to be needed, the old roster's dead, and the records are gone. RAND might very well be concerned with the necessity of having a really selected roster in the social science field, probably under Trytten's office in collaboration with others. There is, for example, the Hattery study of the sources of information about scientific personnel, which is a first step that should be followed along.

Marquis: The Army is paying for a study of key personnel in scientific fields; perhaps we should recommend through the Air Forces that that include social science personnel. It seems to me quite appropriate.

Weaver: That proposal arouses my enthusiasm just as completely as the preceding one did not. I think we are learning how to do this well, and are in the way of getting a really useful and working roster in the field of the natural science. We are going to need it desperately in the field of the social sciences. As this aspect of RAND develops, it would be completely natural and important to include a recommendation to the Army Air Forces that this already existing effort be supplemented in that direction.

Stouffer: The state of social science today is such that practically all the competent social scientists are being produced by no more than 10 university departments in the country. You get a great mass of very second-rate people coming up from other places, and maybe now and then a diamond in the dust. The roster we had in World War II was completely worthless. It just collapsed under its own weight. If I were in RAND's position, I would ask 10 people who the bright boys are. Incidentally, they probably would not be the boys that are on the roster at all; they'd be graduate students just about ready to complete their work. They are a hell of a lot brighter than their professors in the present stage of social science. At the present stage of social science probably not more than 10 universities are turning out the men in whom the Armed Forces would be interested for the top 200 jobs in the country. There are people in those universities whose judgment could be relied upon, who could evaluate those people. I would never go to a roster to locate these people. I'd go to the 20 people and locate them that way. It is a different situation in chemistry and mathematics and these older established fields at the present time. And I'm not even sure I'd go to the 20 people. I'm not sure but what I'd go to Don Young and two or three others. That might not be quite true in the field of psychology. It is certainly true of sociology, economics, political science and anthropology. I think you can ask 10 people and get all the information you want. Why spend a lot of money on setting up an elaborate card file system and keeping it alive? What a job that is!

Stouffer: You get these big questionnaires in and you never can keep the thing alive once you've established it. It's a terrible thing. I'm not too sure that in the present stage of social science it isn't much better to play by ear.

Young: I want to register an exception to that. I think Stouffer is talking about the old National Roster, and if he is I would agree 100% -- it was worthless. But that's not what we are talking about, unless I misunderstand the kind of roster that Trytten keeps. It's true that you'd get a much bigger one in the natural sciences than you would in any one of the social sciences, but I do not believe that you could ask any small group of individuals and get the kind of information that Stouffer wants now. You can not do it, for example, in the field of economics, where there are 10 departments turning out pretty good economists, and where your specialists are such that no five men know the field and the qualities of those people. I think that goes for some of the other areas as well, for example, anthropology. If I asked Ralph Linton about an anthropologist I'd get a totally different reply than if I asked Robert Redfield, and they are both good men. A small roster which would make a particular effort to go down into the younger group would be something of real value.

Helmer: I'd like to approach the problem from a slightly different aspect: the question of whether the training of scientific apprentices should be encouraged -- so that, for instance, work at RAND would give them college credit toward their degrees.

Goldhamer: With respect to social science research, the problems that RAND would have to face are not going to be solved simply by having a group of bright persons who represent the cream of social science. The bright people whom one might skim off the better universities are possibly the persons who ought to direct the studies, but the actual work is going to have to be done by persons who in many instances may not even be scientifically trained, but who have very special fields of knowledge. Suppose, for instance, the question arises of trying to manipulate the Roumanian population in order to wean it away from Russian influence. It's true that the framework of the study would be put together by someone from the university. Ten to one that that person will be totally ignorant and totally incompetent actually to execute the research. Probably what you'll need is some person who has come from the slums of Detroit and has managed to get to the university, who knows Roumania perfectly and perhaps even came from Roumania himself. The exploitation of that type of personnel is extremely important to RAND.

Stouffer: I agree with you completely on that. There is no conceivable roster that would get this kind of expert; that's why I'm registering at least a tentative opposition to the roster proposal. There are two types of social science personnel that you are primarily concerned with -- top management and brilliant youth. You are going to get your top management personnel always by talking to the man who knows him, and never by getting him out of a card file. The brilliant youths are never going to be in a card file. In social science there isn't a hell of a lot in between that you could do much with.

Goldhamer: About the card file, I think I would agree pretty much with you, although I don't think it is entirely without use.

Young: We had some money from the Rockefeller Foundation during the war for what we

Young: called "demobilization fellowships". By chasing around the universities, the OSS, the State Department, etc., we dug up the names of almost a thousand people in the social sciences who were recommended most highly by leading social scientists and administrators for whom they had been working. Of that number we appointed about 175 to fellowships. They are the kind of people that we have to depend upon, and there aren't any 10 universities you can go to and locate those 175 men. I want to repeat that Trytten is not talking about a card index. He isn't just circularizing by mail and then putting whatever he gets back on cards. This is a very carefully analyzed personnel record that he is going after. He is avoiding the mistakes of the old roster, of judging success by whether you got a half million cards or a million; and he is not depending on the punchcard system.

Dollard: You don't dare let your selection rest on the word of 10 experts in universities. But a review board to make some qualitative judgment about your personnel is highly necessary. That was what was really wrong with the war-time roster, that it classified men who had done distinguished work in social science with men who were just pip-squeaks. They were all sociologists or psychologists on record. You could have a much better roster than we had, and still stick to the roster system.

Weaver: When we try to find people in the Rockerfeller Foundation, about 80% of the time we depend upon a relatively small number of individuals that we know have ability and common sense and judgment and that curious sense of honesty which leads them to say what they really think about somebody. But it isn't all we need; a lot of the time we have to go out and beat the bushes by sending our representatives to the University of Utah and the University of Nevada and the University of New Mexico and every other place in the country and sit around and talk to the young fellows.

Marquis: In the psychology work during the war we used both methods. In general, we found the roster information seldom useful, except in one sense -- it would provide a list that would serve to jog our memories about people whom we would otherwise have forgotten.

Young: The roster could be used not only by RAND but by other agencies. It would serve as a basis for any social science job, either of application or research. And it is something that could be stepped up when an emergency came and we have to go a little deeper into the barrel, without having to do it blind as we did this last time. Obviously it is a problem for investigation, if there is such a difference of opinion around the table.

Weaver: I judge that most of you feel that there exists at the present time a reasonably effective set of efforts to study the general problem of recruitment and training of personnel, and efforts to assess past, present, and possibly future needed numbers. There remains the question of the possibility of suplementing the new, small, highly improved rosters which are being created in t e natural sciences to include the social sciences. On that subject there is a clear difference of opinion, some thinking that it would be useful, others highly doubting whether it would be very effective.

Marquis: I don't agree with your first statement. I don't think that we're doing an at all adequate job in the recruitment and training of social science personnel, from the standpoint of the RAND problem particularly.

Weaver: I meant that in the assessment of what the problem was, and awareness of and concern for the problem, there were four or five agencies that had done a pretty good job. The country has not activated their recommendations, so we haven't gotten there yet. But as far as having a reasonably intelligent and comprehensive analysis of the problem is concerned, it seems to me that we've got that.

Young: Unless we have something planned out and perhaps operating at low gear in peacetime, we're going to get stuck.

Weaver: That's right. A small roster well done, as you suggested, would have great merit.

Wilks: We went to the roster and got the roster to certify on the basis of careful, elaborate tabulation that there were no other men in the country but these 12 of ours who could perform our operation. They swore to that, and that so impressed General Hershey that we got our men deferred. If it hadn't been for the roster I don't know that we could have done that. I really think the roster served a very useful purpose there.

Young: Project 12:2 is on "Translation of RAND Recommendations Into National Policy" -- "the study of the most effective ways of presenting scientific conclusions and recommendations so that they will be appreciated by policy makers." There isn't anything more important, but it is not the kind of project that RAND should undertake.

Mosteller: No organization with the scope of RAND can afford to forget about the selling of its material to the people that it's working for. If it can't sell its material, it's rather foolish for it to do the work, particularly since the work is usually going to be a little too technical for the people it is working for to understand. It is very important that the people that any such organization is working for be shown in what way the work they are doing and have done can be used in the problems that their employers are interested in. In so far as that is necessary, it is necessary to learn how to sell. Whether to sell as a national policy or as something to be considered is another matter. But selling itself is important to any organization.

Young: I don't think that an agency under contract with the Government should go into a project of trying to find out how it can make itself more convincing at the policy realm. The problem is an important one, but I think as a matter of policy it is out of the RAND's field.

Kaplan: Would you be interested in a project which would bear on the same problem, but raising the more general question of the role of the advisory function in the determination of policy?

Young: I have had quite a number of sessions on the subject and have not gotten very far. There is something there and we don't know how to get it out, but I don't think it belongs to RAND. I wish it did; I'd like to see somebody working on it.

Mosteller: How about a slightly different title -- "The Transmission of RAND Recommendations to the Policy Level" -- or something like that?

Dollard: That's an administrative problem, but a terribly important one. The problem is different depending on how scientific the material is and how basic the research is. If you are, in effect, answering questions that the policy maker knows that he has, and in terms that he understands, it is relatively simple. If you are having to call attention to the fact that he hasn't phrased his questions right, that he really doesn't know what the decision is that he has to make, it is terribly difficult. This winter some of us tried to get up a book representing the experiences of social scientists in war-time in transmitting research findings to the policy level, and we fell down just on that point. There was so much difference on what was scientific material and what were scientific conclusions that the thing just collapsed. I can give you the names of two or three people who were very actively interested in it though. One illustration: The War Department was inclined to ask such questions as this: How could we make our troops respect their British allies? Translated into research terms that question became -- "What factors affect the attitude of the Army personnel toward the British?" It was very difficult to get the Army policy makers to worry about that problem. They would say, "There is a war on and we need the answer." The real question that you haven't stated here is how you can get in on the definition of a problem early enough so that you can educate your policy makers to accept a research statement of the problem, and therefore be prepared for a research answer. The thing really goes back to the planning of the research. If it doesn't start there you are licked.

Goldhamer: If RAND in one of its researches unearths findings that would radically alter the evaluation that the policy makers now place on what they are doing, certainly RAND might as well close up shop if it is not going to get those findings across to the policy makers in a very effective fashion. One of the projects that was discussed this afternoon might be a very good illustration of that -- on the impact of American public statements on foreign attitudes. Presumably those statements are made by public figures with the intent of getting some sort of response which they consider desirable. Suppose, as a matter of fact, one finds in these researches that they're having quite the contrary effect. Isn't the problem that we are discussing here simply one of telling the policy makers that they are not carrying out the policy they think they are carrying out, but are doing the exact contrary? I don't quite see that that arrogates to RAND a function that in any way lies outside its scope.

Weaver: There are two problems here. One is the problem of internal administration and direction of RAND. Relative to that, it is clear they've got to do an intelligent and effective job of their own reporting. Unless they do it, the thing is going to be just to that extent ineffective. That is a perfectly real problem to them. There is no question about that. On an entirely different and much broader level, one can ask general questions which get into all sorts of ramifications, including practically all known psychological and pedagogical facts about visual education, chart making, etc., etc., of how research results can be presented most effectively for a specific purpose and an assumed audience -- because the problem doesn't become defined until you have both of those. I don't really see that second problem is one which is sufficiently relevant to RAND's general objectives to warrant RAND's going into that problem on any broad scale.

Dollard: To start with, one might make an analysis of our war-time experience. We just came through a situation in which a great many people have had a frustrating experience in just this area. Certainly that must have been true in the physical

Dollard: sciences as well as the social sciences. I can testify that it was true in the social sciences. This is a problem that has concerned a good many social scientists who were in war service. We attempted this year to work through it and the difficulty we encountered was the question of what is scientific work. Some of the men who thought they had solved the problem most skillfully had, in fact, retreated from being scientists and become policy makers. They didn't know that, and thought they were being very successful. What they had really done was to go intuitive, and compete with the advertising men and the movie makers that they were supposed to serve as research men. That's a problem that you are going to run into, in the social if not in the natural sciences, as soon as you try to analyze it.

Wilks: One thing particularly important to keep in mind here in connection with social science findings which are not in themselves definitive is that the presentation of facts without recommendations is ineffective. The trouble was that the facts contributed maybe 10% of the information that was necessary for making the recommendation. The other 90% was made on the basis of a hell of a lot of other stuff. The policy maker wouldn't look even at it unless he could say, "All right, what should I do now as a result of these facts?" If you tried to say, "These facts are one thing you should take into consideration; but there are nine other things to take into consideration," he'd throw you out of the office. He didn't have time to monkey with that sort of stuff. That was our universal experience.

Weaver: I'm including among the possibilities that the most useful thing would be to make a critical examination of the war experience. Perhaps research isn't the right word to apply to that, but a critical examination of it is one of the possibilities. With that broad understanding I think there was something better than half of you that thought that something could be done.

Wilks: Project 12:3 is on "Security Classification of Material" -- "a preliminary survey on developing a rational set of criteria for determining what information should be classified for security and what degree of classification should be imposed in terms of long-range national welfare." It's a good thing, but it should not be done by RAND. It should be done as an ONI or Army Intelligence task. RAND can either work in very close cooperation with them where they really take the leadership, or let them do it themselves.

Young: They may not take an objective view on that problem.

Wilks: I know, but those are two very elaborate, long-standing organizations. I doubt if it would be possible for some outside organization to come along and develop a theory of classification that would get any acceptance whatever from them, unless you work along with them and gradually convince them.

Young: That is being done in some fields. Atkinson, for example, working under a grant from the NRC and the SSRC at the Library of Congress, has been getting enormous numbers of maps and other materials unclassified. But he is working, as you say, right along with the various Government agencies.

Dollard: I know only my own division in the Army where, according to Army regulations, classification was a matter of the judgment and the conscience of the individual officer, as the classification officers never classified any documents. That was done by whoever produced them. Declassification was a somewhat different matter, but you

Dollard: had fairly good control of your material to start with. Is that generally true in the Services?

Weaver: No, there are many areas in which the levels of classification are set up on the basis of artificial initial decisions. If the management of RAND or any other such organization could convince the High Command in the Army or the Navy that it was worthwhile to have a careful and scholarly study made of the problem of classification and security and national welfare, it would be a wise thing to do. There have been some incredibly stupid things done in security. But I simply can think of nothing that seems to me a more completely futile waste of time than for any organization to decide to do that because of its own interests and its own initiative, unless the move came initially from outside. I have knocked my head against that wall for six years already, and I don't intend to do it any more. If they would say, "It may just possibly be that the classification theory on the whole Manhattan project was not the best possible. Maybe we better study some of the factors that really operate in this whole field of classification and security. Will you get some good people to do that?" Then I'd be very enthusiastic about it. But I would not give two whoops in hell about it unless it started there. This is a commodity which I think is not salable. But if you could get an agency to think it was its own, and that the project was being carried out under its own auspices, I think that would be wonderful.

Kaplan: Would you think that the possibility of such an agency being interested in revaluation of the classification procedures would be greater if there were available to it, on some other than a purely impressionistic basis, a concrete record of the respects in which the system now might be judged to be inadequate? You have two different possibilities. You have the possibility of sitting back and waiting and perhaps making some suggestion in the hope that someone might be sufficiently interested. And you have the possibility that is indicated by gathering objective data, perhaps by canvassing such men yourself and asking, "What has happened to you during these six years in which you have beaten your head against the wall?" Can we exhibit your bruises and say, "Look what you've been doing to poor Weaver; do you want to go on doing that?"

Weaver: No, I don't think that will do it, Kaplan; I really quite seriously think that won't do it. I don't think any amount of effort will do it. It might be possible for somebody who has a very high position of trust and influence -- Bush could raise the question or JRDB could raise the question with the control officers in the Army or the Navy. If I were going to do it I'd start out by saying, "By the way, gentlemen, are you perfectly happy and contented with the way this thing worked during the War?" If they said yes, I would go off and say, "I'll come back a year or two later and ask again." If they said no, then I would follow it up and say, "Well, that's interesting. What are you doing about it?" and get them to ask me to do something. Perhaps it is a piece of selling technique, but unless I could sell them the idea that they wanted to do it, and they wanted to start it, and it was being organized under their auspices, I wouldn't spend five seconds on it.

Mosteller: I never heard a better endorsement of a subject.

Weaver: Well, maybe so. I think the essential content here is awfully important. We made some very stupid mistakes during the war.

COMMITTEE V
METHODS, ORGANIZATION AND PLANNING OF RESEARCH

F. Mosteller (Chairman)

C.I. Barnard
C. Dollard
O. Helmer
L.J. Henderson Jr.
C.L. Hull
D. Marquis
S.S. Wilks

Mosteller: Of the fifteen projects that were discussed in the Panels of this Committee, there were two which met with considerable approval -- 11:1 on Composition of Research Teams and 12:4 on Project Evaluation with Respect to Alternative Types of War. Those were the only two that really met with any enthusiasm. There was one project which was proposed late, namely Stouffer's on psycho-social measurement.

Marquis: That recommended that work be done to develop, refine, and sharpen methods in social science. I should like to second it for this reason: that a lot of the content in this field is temporary and evanescent. Even if we verify that 76% of the people disfavor Russia, for example, that is a fact which does not have significance three years from now -- it doesn't become a part of the cumulative body of science. It was the experience of a lot of people during the last war that the major contribution of the social science to the war research effort was largely in the trained personnel and methods which were brought to new problems. There was very little verified principle which was taken over from social science and directly applied. For example, when we looked at a training program we couldn't go back to the books and find principles which could immediately be applied to revising the program. What we did have were methods by which we could analyze and evaluate changes, and some variables which were isolated to the extent that we knew it was significant to look into them although we didn't know what weight to give them. It might therefore be that in planning relatively long-range work -- something that would be useful ten years hence -- the most valuable thing we could do would be emphasize methods which could be applied to the topical problems of that moment.

Hull: I'd like to reinforce these words of Marquis. One of the methodologies that is in need of considerable improvement is the matter of quantification. I would like to put on record that the quantification should be and can easily be something more than the percentage sort of thing which we get in the ordinary polls. Some twenty years ago Thurstone worked out a method of quantification in which a well recognized principle on the variability of the subjects, called involuntary action potential, was used as a unit. It's rather laborious, but it does give results. In addition to quantifying these things way beyond the ordinary percentage method used by the polls, there ought to be a zero point. Suppose, for example, one wants to find out how strongly the Americans dislike the Russians, or how strongly anybody dislikes castor oil. I suggest that it would be possible with sufficient ingenuity and care to get a zero point so that it would be universal, wouldn't change with momentary feelings

Hull: -- some simple thing like how people dislike small pains or shocks or pinpricks. It's hard to believe that a pinprick response would be shifted by the excitment of a war scare. So I would like to put into the record that these people not only quantify, but get an absolute zero that would not change, so that one could compare the strength of the dislike of Americans for the Russians now with the dislike of some other group of people, say. I think it is feasible and should be tried.

Dollard: Concerning Project 11:3 on a Reading Machine, a fairly comprehensive program of research on its development was gotten under way during the war and is still running, but will shortly be in need of refinancing. It is probable that the government agencies which originally did the financing will not keep on with the project. Haskins has been working on it during the war, originally as a means of providing better guidance devices for blinded soldiers. Having solved that relatively simple problem from the standpoint of engineering, he proceeded to the much more complicated problem of the reading machine. He has a young psychologist from Yale, I believe, and my impression, which should be verified, is that it's a very good research team. Haskins, I would say, is a highly competent man. It is going on in Haskins' laboratory, which is a private laboratory here in New York.

Henderson: I think it would be useful for us to try to discover what the actual situation is to see what can be done.

Dollard: I know from conversation with Haskins that his estimate of the time needed to get the result is another five years with this team.

Marquis: Another aspect of this gadget problem is just the reverse -- speaking into the machine and getting out a written record. This is of more interest to social science, since 90% of social science is talk and conference and dictation. The development of a voice operated typewriter would be useful.

Helmer: Such a device would be particularly helpful in monitoring radio speeches, particularly propaganda from abroad.

Wilks: On the Composition of Research Teams, operational research was quite successful during the war, at least in some parts of the military organization. The thing I am particularly interested in is whether there is not quite a lot of material that might be gotten out of that experience and made available to industry and various other regions of activity. There is some enthusiasm and interest at least in some quarters for seeing more done along this direction. As you know, there is a peacetime continuation of these operational research groups in the Army Air Force and the Navy. There is a considerable amount of discussion going on right now for setting up a group; incidentally these groups have become known as Operations Evaluation Groups. I don't know what the Army is going to do about it; but they certainly are going to do something about it, I should say within the next year. I have recently talked to the Army Air Forces group and the Navy group about this general question whether there's something that ought to be got out and presented a little bit more generally, and they are very much interested in seeing that done. There are several indications of the eagerness with which the Services view this thing. I have talked to various individuals about whether something could be done toward bringing together a small conference or committee on this, perhaps through the National Research Council, and I have found some interest from the National Research Council in it. The Division

Wilks: of Physical Sciences is quite willing to sponsor an exploratory conference at which you would have people representing the Services, industry, and the universities to discuss whether there is something here that could be carried further -- studying the methods and organizational procedure that's involved in this activity. There's certainly interest on the part of everybody in the Services to do something, but nobody takes the initiative. The situation is getting ripe for some independent group to assist in bringing efforts together. I don't think it's particularly a research problem. It is just a matter of bringing this experience out and making it available. Nobody has brought out a report or statement that would be endorsed by all the groups describing how operations analysis works. I have a very good suspicion that if you talked to people in industry about this thing, everyone of them would say that they have been doing that for twenty-five years. The question is have they, and have they fitted it in organizationally so it would work most effectively?

Henderson: I don't think that those people in industry who are most puzzled about how to improve their operations would do anything but welcome suggestions for improving the handling of that kind of problem.

Wilks: In the next war, if there is one, a great deal more of this kind of group activity would go on. It would be well to know what are the characteristics of a good operational research group, what is the procedure for getting it set up and going. In the last war there were all sorts of difficulties in getting that type of activity going. It would be desirable to have this whole story out somewhere, so that people who set these things up would know better how to go about it next time.

Mosteller: Do I take it that your concrete proposal is this: that RAND, the Army, the Navy, industry and universities should get together on the general problem of operations analysis as developed in the last war as a technique widely applicable, not only in industries, but also for solving both general and specific research problems, that this should be some kind of a cooperative venture, and that RAND should take some initiative in making sure that this general cooperation is achieved and something set in motion at an early date.

Wilks: That's the entire situation. Somebody ought to do it. I don't know whether somebody other than RAND could do it. Possibly the National Research Council ought to initiate it. I am simply bringing it up here as a very important thing -- in my opinion, and also in the opinion of people in the Services.

Marquis: RAND should look at the experience of these groups in the war to see what social science problems can profitably and effectively be attacked by such means. There is something more to it than just inter-disciplinary teams. There is a type of systems analysis that has not to my knowledge been applied in the social science field. It is a fruitful approach, and I should like to see an imaginative person like Wilks look at these various social science problems that we have been talking about, or look beyond them, and see if there aren't some which could be attacked by such means. Certainly shipping problems in economics are of that type, the mobilization of a division, and the assignment procedure in the Army. Those could be nicely attacked by operational analysis.

Mosteller: The crisis and disaster project discussed in Panel 1 is a good example

Mosteller: of a study that just isn't going to be any good unless you have some systems analysis to tie it together, and somebody in there studying it from more than one angle. It has the advantage that it is a peacetime operation, that will have developed and be ready to merge into a wartime situation, should one occur. Do I take it that it is the consensus of this committee that this is a very important problem, that RAND should discretely assist in whatever manner it can, that the social scientists here should take an interest in the problem from whatever view they can, and that it is not settled whether RAND should attempt leadership in this program or rather support it financially or with administrative assistance, or liasion or any other way. You are nearly unanimous that this is correct. Barnard had remarked in another panel that for his organization operations analysis was now a routine and well-accepted method of research. Is that right?

Barnard: Without using the term 'operational analysis' I think that is essentially correct, that is the way we do work. It is the way we have to work, because we're dealing with a vast system of many inter-dependent variables -- human, mechanical, and electrical. It is necessary for technological reasons, as well as the fact that practically nothing is done in telephone operation in which two or more people are not simultaneously concerned.

Mosteller: How widespread in other industries is the use of such techniques?

Barnard: I don't know the answer, because I don't know it sufficiently well from this point of view. In a much less explicit way, I think there must be a great deal, but I doubt if there's much that is explicit. Most of the organizations don't know how to operate a laboratory, for instance; they have what they call a laboratory, but they're tinkering propositions.

I would like to make this comment about these projects. Again and again you have listed here as projects the kind of things that bear on management decisions. Usually it is impossible to get anything but qualitative stuff, a list of the factors that may be involved or appear to be involved, and beyond that you can't go anywhere. There's no chance in the immediate future, under this program, to do anything in the way of really scientific work. Nevertheless that kind of analysis or research is very important to a thing like running an Army or telephone business or anything else. A scientifically trained mind, an analytical mind with a background of scientific experience and knowledge can be, as it was in the war, applied to very practical questions that are not scientific in character. It's a case of application of intelligence, of which we need a great deal more than we've got. I don't think we can possibly eliminate the role of judgment on certain practical questions -- it is a matter of just intuitive judgment on the part of people who demonstrated that that kind of judgment is good in them.